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DEMONSTRATION OF MR. HUNTER'S LENS TESTING APPARATUS.

MR. T. SMITH gave a demonstration of Mr. J. de Graaf Hunter's apparatus for testing the definition of photographic and other lenses. This apparatus consists essentially of two discs mounted on a common axis and capable of rotation together. The arrangement of these discs is shown in fig. 1. One of them, shown by the

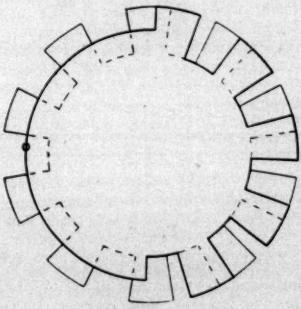

FIG. 1.

thick lines, has for one-half of its circumference an accurately milled semi-circle, and on the other half a series of sectors separated by spaces, the spaces and sectors being equal in width to one another. The second disc, which is in contact with the first, is shown by the fine and dotted lines; this has sectors and spaces

similar to those of the first disc, extending over the whole of its circumference. The sectors of the two discs can be given any relative angular displacement while the discs are rotating. For testing, a microscope is used to examine a part of the image of the disc formed by the lens, the definition given by which is to be examined. An idea of the amount included in the microscope field will then be given by the size of the small circle shown on the milled part of the disc in fig. 1.

When the discs are rotating we have our field of view illuminated in two ways, alternating with one another; these are shown in the upper and lower halves of fig. 2. The shaded parts

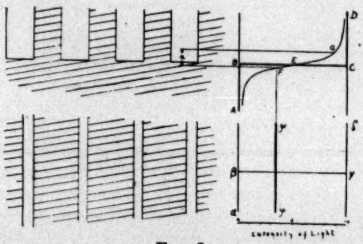

FIG. 2.

show portions of the object which are dark, and the unshaded portions are uniformly bright. We are to imagine these objects drawn so rapidly across the field, that we gain the sensation of a constant illumination. In the actual *object* for the first method of illumination we have one-half of the object quite dark and the other half uniformly bright. This is represented diagrammatically by the curve A B C D of fig. 2. In the *image* of this object formed by the lens under test, the change from the light to the dark portion of the field will be gradual,

and the illumination will be represented by a curve such as F E G. The width of this transition band serves as a measure of the badness of the definition, and this is measured by tracing the curve F E G.

In the second method of illuminating the field the light is uniform, and is the same in the object and its image. This is shown diagrammatically by the line $\psi\psi$, the distance of this line from $\alpha\beta$ being proportional to the width of the light openings. Of course $\psi\psi$ coincides with $\gamma\delta$ when the light openings have their maximum value—*i.e.*, when equal in width to the dark portions.

On looking through the microscope, we see the two methods of illuminating the field shown by the curves F E G and $\psi\psi$ alternating with one another, and the effect is that of a flickering light. There is only one part where there is no flickering, viz., at the part corresponding to the point F. This part may be isolated by a narrow slit moved by a micrometer screw in the eyepiece of the microscope. We will suppose, by way of illustration, that $\psi\psi$, and consequently F, is the point where the illumination is 1/3 of the maximum. By making the openings in the lower diagram widen till they are 5/6 of their maximum value, we obtain another point G, where there is no flickering. The distance in the image between F and G—*i.e.*, the distance marked *a*, is measured on the micrometer screw. By means of this measurement we have found that in the image of the edge which serves as the object examined, there is a distance *a* between the points where the light has risen from 1/3 to 5/6 of its maximum value. Other points, or the curve F E G, may be obtained in a similar way.

Several interesting series of observations have been made in this way, and in general it has been found that with good modern anastigmats the definition on the axis is improved by stop-

ping down to $f/8$ or $f/11$, by the cutting out of marginal rays affected by aberrations; but further stopping down impairs the definition owing to the increased effect of diffraction. This, of course, does not hold for the outer parts of the field of the lens, where the definition is in general improved by stopping down to $f/32$, or even further.

A full description of the instrument has been given by its inventor in a paper communicated to the Royal Society.

Efficiencies of Photographic Shutters.

A demonstration was also given by Mr. Smith of a method of testing the speeds and efficiencies of photographic shutters, due to Messrs. Campbell and Smith.

The measurement of the time is made by a vibration galvanometer tuned to resonance with a source of vibrations of known frequency. Light from a bright point source falls on the mirror of the galvanometer, and is reflected as a beam vibrating horizontally, so that in the focus of the mirror we obtain a bright horizontal line instead of a point. This vibrating beam of light passes through the shutter to be tested before it comes to a focus, and the shutter is opened at a suitable instant by a photographic plate as it falls under gravity in the plane where the light is brought to a focus. There is thus traced on the plate a sine curve, in which each complete vibration corresponds to a known length of time (say 1/100th or 1/1000th of a second), and thus, by counting the number of vibrations recorded on the plate the length of time for which the shutter was open is found.

Lantern slides were shown giving full details of the apparatus, and examples of the records obtained, both for duration of exposure and efficiency. A special camera had been used which enabled ten records to be obtained on

one 5in. by 4in. plate, the time required to do this with a between lens shutter being about one minute.

A detailed description of this method is contained in a paper contributed to the Physical Society of London.

THE THEORY OF VISION AND COLOUR PERCEPTION.

By F. W. Edridge-Green.

Lecture delivered on October 21st, 1909.

Mr. President, Ladies and Gentlemen,—I assume that the cones of the retina are insensitive to light, but sensitive to chemical changes in the visual purple. Light falling upon the retina liberates the visual purple from the rods, and it is diffused into the fovea and other parts of the rod and cone layer of the retina. The decomposition of the visual purple by light chemically stimulates the ends of the cones (probably through the electricity which is produced), and a visual impulse is set up, which is conveyed through the optic nerve fibres to the brain. I assume that the visual impulses caused by the different rays of light differ in character just as the rays of light differ in wave-length. Then, in the impulse itself, we have the physiological basis of the sensation of light and in the quality of the impulse the physiological basis of the sensation of colour. I have assumed that the quality of the impulse is perceived by a special perceptive centre in the brain with the power of perceiving differences possessed by that centre or portions of that centre. According to this view the rods are not concerned with transmitting visual impulses, but only with the visual purple and its diffusion.

I shall now show you some pictures of the rods and cones. The point that you will notice here especially is that the rods and cones

have absolutely separate functions. In the centre of the eye where we see best only cones are to be found, and we can trace these cones very definitely to the nerve fibres which convey the sensation of light to the brain. But when we come to the rods we find something very different. We find that these rods, each of which contains the visual purple in its outer segment, are very loosely connected with the optic nerve fibres, and instead of one element being connected with a nerve fibre, as we find in the case of the cones, we see that a great number of rods appear to be connected with one nerve fibre.

I must now refer to the beautiful and accurate work of Kühne, who has done so much in connection with the visual purple. He has been very much misquoted, as will be noticed from the few extracts which I give. I am not aware of one of the facts discovered by him which is inconsistent with the hypothesis I have given; I will even go further and say that they all directly support it. Kühne expressed the opinion that the visual purple could not be essential to vision because it was not present in the cones, but he does not appear to have looked for it between the cones. It seemed to me more probable that the rods should produce a secretion which would affect other cells rather than themselves. In one case Kühne did find the visual purple diffused into the rest of the retina. He writes:—"In one of these eyes which had been laid open in the dark for an hour, I saw to my great surprise the whole of the retinal mass flooded by a clear purple solution, which, when poured upon a plate, exhibited the same behaviour to light as the mass itself." This refers to the retina of a shark.

Kühne's chief objection to the view that the visual purple is the visual substance—namely, that is not present in the cones—is a necessary requisite of the theory which I have advanced.

The facts which he has brought forward in support of his other two objections upon examination will be found to support my theory. They are that animals, such as frogs, naturally possessing the pigment, continue to see when their visual purple has been absolutely bleached, as it may be by prolonged exposure to strong light, and that the visual purple is entirely wanting in some animals which see very well. Now, as I said, Kühne has been very much misquoted on this point; even by well known physiologists. Physiologist after physiologist has said to me, "But how can the visual purple be the essential substance of vision when it is not present in the daylight at all?" Again I will quote Kühne, and you will find there is the very greatest difficulty in getting rid of the purple even in daylight, except by exposing the eyes to direct sunlight, but you can hardly call that a physiological experiment. Kühne writes:—"Without direct sunlight, I have never succeeded in a room in obtaining frogs' retinas free from visual purple, not even when I allowed the animals to leap about before the window the whole day long." He bleached their eyes by exposing them for 20 minutes or half an hour to direct sunlight. I should not like my eyes exposed to direct sunlight for a few minutes. My contention is that in those cases in which the retinas were bleached by sunlight, and the frogs were still able to see there was sufficient visual purple or its decomposition products for vision, but not enough for external recognition. A very important point is mentioned by Kühne, namely, that after the frog's eyes had been bleached in this way the regeneration of the visual purple was defective. It had permanently injured the animal's eyes. Although it was able to see to get out of the way of the hand, it did not show that the eyes were in a satisfactory state after being exposed. I will again quote Kuhne on the second objection. He supplies me with the argument I use against

him. "According to all appearances the visual purple in the bird's eye is deficient in proportion as the retina is provided with other more stable means of absorbing coloured light, I mean the coloured globules of the cones. This is at least the case in the nocturnal and predaceous birds, and is entirely true as regards the pigeon and the fowl." It will be seen that this is in favour of my view, the cones having other means of stimulation, the visual purple is not required. Light acting on these cones with their coloured globules caused sufficient stimulation, and the elaborate process, which is at its perfection in man, was not required in order to produce a sensation of vision. Birds having coloured globules had a different mechanism of stimulation. Whether the globules act by absorption or as filters, is not known, but their presence is quite sufficient to explain the absence of rods in these birds. It seemed to me that the photo-chemical processes necessary for vision must have an elaborate nervous mechanism which could only arise from the retina. The peculiar arrangement of the rods seemed to indicate that these were the elements involved. I will quote Kühne on this point:—"It must not be forgotten that the retinal epithelium of the choroid has been proved to be a very important physiological or chemical constituent of the retina, or, stated shortly, as a purpurogenous gland, the cells of which can scarcely fail to possess a very peculiar complicated innervation. Now, I do not see that irritable fibres can arise from any other source than from the nervous mass of the retina." It does not appear to have occurred to Kühne that the rods were the nervous elements for which he was seeking. He looked for some direct connection between the rods and the pigment cells. We know that in other parts of the nervous system it is sufficient to have contact between two cells without direct continuity.

The anatomical arrangement of the retina is

consistent with the theory I have given. In the fovea of the retina only cones are to be found. Immediately external to this each cone is surrounded by a ring of rods. The number of rings of rods round each cone increases as the periphery is reached. The outer segments of the cones are situated in a space which is filled with fluid. The external limiting membrane retains this fluid in its place. So that we have actually in the eye a layer of fluid which is confined definitely by two membranes, and in this fluid there are the ends of the rods containing the visual purple and the ends of the cones, so that the visual purple passes into this fluid. Another very curious and important fact (which puzzled Kühne) was this, "that there are pigmented processes passing by the sides of the segments containing visual purple." These processes, as he thought, were to protect the visual purple, but he could not understand if the rods were to act as a means of conveying a light impulse to the brain why the visual purple should be specially sheathed. But if you take my view, you will understand why it should be protected in the day-time. It is easy to understand how adaptation to darkness is brought about by a steadily increasing percentage of visual purple being added to this fluid, thus rendering it more and more sensitive to light. It is easy to trace the connections of the cones with the inner layers of the retina, but the rod fibres appear to end in nucleated enlargements. Now I shall mention a few facts which are consistent with this view, and absolutely inconsistent with any other view. The first one is that if the cones of the retina are insensitive to light and sensitive to chemical change in the visual purple, light should be able to fall on the eye without producing any sensation of light—that is to say, the centre of the eye which has not been prepared by visual purple should be blind. This experiment is very easy to make. If the cones are not sensitive to light, a ray of light

falling upon the fovea alone, and not upon the adjacent portion of the retina containing rods, should produce no sensation of light, provided that there is not already any visual purple in the fovea. It has been known to astronomers for a long time that if a small star in a dark portion of the sky be steadfastly looked at it will disappear from view, whilst other stars seen by indirect vision remain conspicuously visible.

The following quite simple experiment shows the same thing. If a piece of black velvet, about three feet square on a door, have a pin put in the centre, and the source of light be behind the observer, the pin will be brightly illuminated; and on looking at it (the observer not being too close), and keeping the eye quite still, the pin will disappear, the visual substance diffused into the fovea centralis being used up and not renewed. When viewed by indirect vision, it is impossible to make it disappear in this way. When I have taken great care to have very dark surroundings, and have used only one eye, I have made moderately bright lights disappear in this manner. The greatest difficulty is found with red light, which it is very difficult to make disappear. This, however, we should expect from our knowledge of the visual purple, for red light bleaches it very slowly, and therefore a much longer time would be required before the visual purple already in the fovea was used up. These facts have been attributed to a defective sensibility of the fovea for feeble light. The important point, however, that the light is at first seen most clearly by the fovea and only subsequently fades, has been overlooked. If these facts were due to a defective sensibility of the fovea the star or light would not be visible at first. It is mentioned by Helmholtz, and stated by him to be quite inexplicable that a perceptible interval elapses before we see with our yellow spots after opening our eyes. If, in a dimly lighted room one eye be directed towards a uniformly illuminated surface, then

on opening the eye the region corresponding to the yellow spot will be seen as an oval black patch, and light appears to invade this patch from without inwards. The fovea centralis is the last point to convey a light sensation.

The lecturer then had thrown on the screen slides showing a table of wave-lengths, a section of the fovea and every part of the eye. Proceeding, he said: I have seen the visual purple in the fovea between the cones. Von Kries has endeavoured to show that the rods have a special function, being concerned with vision at low intensities. The observation upon which he has specially relied is that the recurrent image is not seen in the fovea. The method which he adopts is open to the objection that if the recurrent image were retarded on reaching the fovea it would give the same appearance as if it passed it. I have carefully observed this phenomenon, and am quite unable to convince myself that there is anything more than a retardation of the recurrent image at the fovea. Hess holds the same opinion, and describes a simple experiment which appears to be very conclusive. If a thin strip of cardboard be moved backwards and forwards in front of the eye before a dark background, it will be noticed that the recurrent image of the paper will appear to be bent at the position corresponding to the fovea.

If we suppose that visual purple is the means by which we see, it is easily recognised that there must be a definite arrangement for adaptation to light, and this probably corresponds to the forward movement of the processes of the pigment cells which puts them in the most favourable position for renewing and protecting the visual purple, which is used up more rapidly with a bright than with a dull light. If we suppose that in total colour-blindness there is defective light adaptation through an imperfect forward movement of the processes of the pigment cells, we shall have all the phenomena which are found in these cases. The diminution of visual

acuity only occurs in light of ordinary intensity, and vision by a feeble light is as good as with normal sighted persons. The photophobia which is found in these cases would necessarily follow from the imperfect action of the pigment cells. Von Tschermak, Hering, Hess, Garten, and others have found only gradual quantitative differences in the sight between the foveal and the extrafoveal area. The Purkinje phenomenon, the alteration of optical white equations by the state of dark adaptation, the colourless interval for spectral lights of increasing intensity, the different phases of the after image, all exist, not only in the extrafoveal, but also, only gradually diminished, in the foveal region. That absolutely disposes of the argument as to the special action of the rods, because every one of the so-called visual purple phenomena have been found in the portion of the retina where there are no rods. In accordance with the theory I have given, we should expect, as these very able men have found, that there would be only a quantitative difference. I entirely agree with those experiments. I have never been able to find any qualitative difference between the extrafoveal and the foveal regions. I will now speak of the chemical analogy. The visual purple gives a curve which is very similar to that of many other photo-chemical substances. We know that with photo-chemical substances the chemical effect is not proportional to the intensity of the light. That is, a different curve is obtained with weak light from that which is formed with light of greater intensity. It is reasonable, therefore, to suppose that the visual purple which is formed by the pigment cells under the influence of a bright light would be somewhat different in character from that which is formed in darkness. Again, from the chemical analogy which I have just given, even if the visual purple were of the same character we should not expect similar curves with different intensities of light. It is probable that both

factors are in operation. This deduction gives an explanation of the Purkinje phenomenon, or the fact that when the eyes are adapted to darkness the point of greatest luminosity is shifted more towards the violet end of the spectrum.

The photo-chemical action of a substance corresponds to the absorption curve. The following three curves almost correspond :—(1) the luminosity curve for the threshold of stimulation; (2) the stimulation value for total colour-blindness; and (3) the absorption curve of the visual purple. (1) has its maximum at λ505, the other two at λ500. The centre of pure green is also about λ505. The neutral point in dichromic vision is about the same point. Assuming that the visual purple is the visual substance, we get a remarkable correspondence in the figures between the visual purple and the actual facts of vision. These facts bring the visual purple not only into relation with normal sight, but with a disease in which we know there is defective light adaptation. Those dichromics who have shortening of the red end of the spectrum have a luminosity curve which is very similar to that of a normal-sighted person with a spectrum of lesser intensity. We have only to assume in these cases either that the nervous apparatus is less responsive or that the visual purple formed at one intensity of light is similar to that formed at a lower intensity by a normal-sighted person. If we regard the nervous apparatus as not being easily responsive to the stimulus we shall get an explanation of a number of these cases, even when there is a considerable shortening of the red end of the spectrum by dull light. We also have an explanation of other conditions, such as erythropsia, or red vision, white objects appearing more or less red. If we suppose that the eye has remained in a state of light adaptation, the visual purple produced being more sensitive to the red rays, objects appear of this colour. As we should expect, erythropsia is frequently associated with

hemeralopia, or difficulty in seeing in the twilight, the eyes being adapted to light and not to darkness. In green vision the eyes have probably remained in a condition of more or less adaptation to darkness, and are therefore more sensitive to the green rays. Some curious facts follow from this hypothesis; for instance, the after-image of black should be green, not white. When the eye becomes more sensitive to the red and violet ends of the spectrum, the after-image of a white object will be purple instead of black. Many of you may have noticed that the after-image of any colour is in the first instance purple. This fact gives an explanation of a pretty little toy, which excited much interest a few years ago, Benham's top. This consists of two sectors, one white and one black; on the white sector are black lines at intervals and at different distances from the centre. When the top is turned the black lines nearest the black disc appear red, this red being the contrast colour to the green, which is seen as the after-image of the black disc.

Every acute observer knows how when the sun sets reds become darker and darker until they appear black, whilst blues and greens remain conspicuously visible. It is the same with the after-image of black; in a feeble light it is white, raise the illumination it becomes yellow, raise it again it becomes yellow-green, increase it further it becomes pure green, try the experiment in the sunlight and it becomes blue-green. The converse occurs when the eye is becoming adapted to light, the eye becomes more and more sensitive to the red end of the spectrum. Gotch and Waller have both written some very interesting papers on the electrical changes in a frog's retina caused by light. We cannot say what is the exact mechanism, but the electrical effects we have obtained already from the retina are quite sufficient to explain the facts. All that is required is some periodicity in the nerve impulses. We should expect that the physio-

logical centre of the spectrum would correspond to the maximum of the absorption curve of the visual purple, and we find that this is the case.

The lecturer had thrown on the screen slides showing the spectrum as seen by different people, and showing the different classes and degrees of colour-blindness, also pictures painted by colour-blind people—the tetrachromics, the trichromics, the dichromics, and one who was almost monochromic. Concluding, he said: In the past there is no fact that I have had more difficulty in establishing than the simple fact that if you isolate a portion of the spectrum it appears monochromatic. These monochromatic regions are very large, an ordinary normal sighted person sees about eighteen of such divisions in the spectrum. The monochromatic region, and its varying size with different persons, I consider to be a fundamental fact of colour vision. Even a small degree of colour-blindness greatly reduces the number of divisions. For instance, a man having trichromic vision sees only half the normal number in the spectrum. Though dangerously colour-blind, he would be able to pass the official test for colour-blindness with ease. The spectrometer I am showing you this evening is constructed so that the size of these regions can be ascertained with great accuracy. Only last year a well known physicist, who had denied the existence of these monochromatic regions over and over again, came to see them, after I had given an address on Colour Vision in Dublin. He said he could see 12,000 colours in the spectrum, and remarked, "I can tell with the greatest ease the difference in colour between the two D lines." He then sat down to the spectroscope and marked out a monochromatic region in the green, which in size was about one-eighth of the whole spectrum, and a little bigger than normal.

Mr. S. D. Chalmers said there were one or two questions he would like to raise. It seemed to

him that the one great difficulty associated with all purely absorption theories was the difficulty of accounting for the change from green towards red, on the one hand, and from green towards blue on the other. Either of these changes might be attributed to the diminishing absorption of a single visual substance, but why should the diminishing absorption produce red on the one hand and blue on the other? A possible explanation had occurred to him that the other properties associated with absorption might have an important influence; for example, on approaching an absorption band the reflection would increase, and it seemed possible that a change in the reflection co-officient or in the refractive index of the substance on the two sides of an absorption band might meet this difficulty. Also, one had to take account of a great deal more than absorption effects of the chemical substance. It was necessary to take account of the possibility of electrical currents in local resonating systems, and one fact seeming to bear this out was that there seemed to be a very loose connection between some of the local nerve circuits and the main transmitting system. In many devices for receiving electrical waves we would note that the resonating system was loosely connected to the main circuit to avoid the disturbance of its free period of resonance. In fact, electrical as well as purely chemico-physical properties might be involved.

Mr. J. S. Dow said that he had listened to Dr. Edridge-Green's lecture with keen interest. All these questions of the structure and behaviour of the retina had a very direct bearing on problems in photometry and illumination, and it was a great help to those connected most closely with the engineering and physical sides of the subject, to hear the latest physiological theories so lucidly explained. He was particularly interested in Dr. Edridge-Green's important statement that visual purple had been found to occur in the fovea between the cones,

and his additional suggestion that the rods were concerned with the regeneration and distribution of visual purple, but did not play a direct part in vision. There were many effects encountered in attempting the comparison of the brightness of sources of light of different colours which could only be explained by the knowledge of the retina; and some physiologists, he understood, had advanced a theory based on the fact that the cones were mainly instrumental in vision at a high illumination, while the rods were the predominant organs when the illumination was very low. Many peculiar effects, such as the Purkinje phenomenon and the fact that at very weak illuminations we could see light but no colour, had been attributed to the struggle for predominance between the two sets of organs. The same theory had been used to explain the fact that a judgment of the relative brightness of two heterochromatic surfaces varied very greatly according to the portion of the retina on which the image of the illuminated surfaces was received. But, according to Dr. Edridge-Green's theory, a different explanation was necessary. Whatever the explanation, however, there seemed to be a sudden change in retinal conditions when a certain order of illumination was reached. According to the speaker's experiments, this abrupt change occurred at about 0.2 meter-candles. When the illumination was reduced beyond this point, for example, the central region of the retina, where there were only cones, seemed to become more or less blind, and there was a sudden fall both in acuteness of vision and in capacity to appreciate changes in light and shade. Perhaps the lecturer could explain why this should be the case. Some work of Professor Burch seemed to fit in with Dr. Edridge-Green's theories regarding the retina very well. Dr. Burch found that, although at very low illuminations one was usually unable to appreciate colour, the faculty was eventually recovered if one rested in complete darkness for

about two hours. This suggested that the regeneration of the visual purple had an important effect. Mr. Dow added that he appreciated Dr. Edridge-Green's distinction between the different causes of colour-blindness. He had always been impressed by the view that cases of colour-blindness might exist the cause of which was some disturbance of the centre of perception rather than some peculiarity of the retina. Cases described by Dr. Edridge-Green and others, for example, seemed to suggest that loss of colour-perception did not invariably entail a corresponding loss of the power to perceive light.

Dr. Edridge-Green said one point mentioned had been raised nearly every time he showed the pictures. He could best make it clear by comparing a normal-sighted man with one who saw seven colours in the spectrum. Suppose, for instance, he wished to test supernormal vision with his lantern. He would put in a glass which he knew would correspond exactly to the indigo. It would have its dominant wavelength in that particular portion of the spectrum. Any ordinary sighted person would call it violet, and every time he came to it he would call it violet. In no circumstances, even by accident, would he call it indigo, and if he tried to guess you would know that he was wrong. As a matter of fact, colour-blind people know that if they guess they will be considered incompetent, and they do not think that they are incompetent. They have always seen in that particular way, and they think they have as good sight as anybody else. Then a man who was guessing would easily be detected, as he would no doubt at one time call the colour violet; whereas a man who saw indigo would name it every time and remember it. Regarding the pictures painted by colour-blind people, a man had an unlimited number of colours spread out on the colour palette. He sees a certain colour on the picture (for instance, yellow), and he makes a

mental note that it is green. Then he looks amongst the paints for a typical green to paint it with, but he would not look at the yellows, which are nondescript colours to him. If I take the most representative colour and say to him, "Won't you take that colour?" he will say, "No, that is a poor sort of colour." Another test for colour-blindness, used in Germany, comprised a number of pencils with coloured chalks protruding. These were placed before a man, who picked out the particular colour he thought he should have to use, and in this way many mistakes were made. He had mentioned that the visual purple had been very carefully examined by Kühne and others. All the facts had been found to agree with visual phenomena. The cones had no visual purple; therefore Kühne and others assumed that visual purple could not be essential to vision, in spite of the fact that it was in every way perfect for that purpose. The visual purple phenomena were found in the centre of the eye, so that any theory of sight had to explain why they were found there. The retina had been examined photo-electrically by Gotch, who found a difference in time relations, the starting interval and the curves were not precisely the same, there was a little difference in the curve all the way up the spectrum.

The lecturer then, in answer to Mr. Dow's question, said that there were three definite processes which had been shown to take place in the retina, and these varied according to the wave-length of the light used. Gotch had shown that there was a latent interval corresponding to the diffusion of the visual purple; the second process corresponded to a photo-chemical change, and the third to a nervous stimulation. A friend of his, who played golf with him, could pass the ordinary official test, but could scarcely recognise a red flag at a distance equal to the length of that room. It took more stimulation for the visual purple to diffuse into the

fovea. Where the rods were concentrated they would expect to get the easiest effect, but where the visual purple had to travel some distance they could obviously see that they required a much stronger light. But just as had been mentioned about Burch's experiment, if we only waited long enough we might be able to see with the centre. Von Tschermak, Hering and Hess say that all the phenomena obtained outside the fovea are found in the fovea, showing no qualitative difference between the extra-foveal and the foveal regions. With regard to Mr. Chalmers' remark, there was undoubtedly a great deal more to do; in fact, when any discovery was made, it opened a number of fresh lines of research. One single substance, the visual purple, by its decomposition caused a sensation of light.

THE PROPERTIES OF PRECIOUS STONES.

By Mr. B. J. Tully.

[Abstract.]

Delivered on December 16th, 1909.

Mr. B. J. Tully had been announced to lecture on the optical properties of precious stones, but he apologised at the outset by saying that he had been under some misapprehension in the matter of the subject, and although the optical side would be dealt with, he would deal more generally with the properties of precious stones. All precious stones, he said, to the exclusion of pearls, were of mineral origin. They were selected for their beauty and rarity, and the quality of possessing sufficient durability to render them suitable for purposes of personal adornment. They were sub-divided as precious and semi-precious, although there was no definite line of demarcation.

The beauty of a gem was not manifest until the cutting and polishing had been carried out. Then its individual colour and lustre became apparent, and it was upon the degrees of colour and lustre that the value depended. Beauty might depend upon both colour quality, as in the case of the rich ruby, or on total absence of colour as in the pure white diamond. The property of hardness was of vital importance, and it was this attribute which rendered the stone able to withstand attrition and knocking about by the wearer. The beauty of a great many precious stones was ruined by rough and improper usage. Rarity was governed by supply and demand, and only those stones which combined great beauty and rarity and high degree of durability laid claim to the title of "precious."

The average person was probably acquainted with eight or ten varieties, but as a matter of fact there were eighty to one hundred varieties

in everyday use, and some one hundred more, more or less, often seen. They were all classified, like the human races, under six different heads, and classified by their chemical composition and physical characteristics, into groups. Those in leading groups showed many peculiarities in a wide range of physical and optical character, and there were one or two points characteristic of all of them, whilst some of the optical effects were most striking. Most of the stones were perfectly crystallised, but some had undergone interesting processes of crystallisation. The coloured varieties were numerous, and many closely resembled one another. This similarity, in many kinds, had resulted in many fanciful names being given to them in order to tickle the palate of the public, causing much confusion in the matter of correct nomenclature. On the physical side, hardness or durability of the stone was one thing and the relative density another. The refractive index and dichroism came under the heading of the optical properties.

The lecturer proceeded to illustrate the various tests used to determine these characteristics, and by which many of the varieties of stones are identified. He also dealt with the different forms of crystallisation. As the optical axes which governed the optical effects of all crystallised transparent mineral generally coincided with their crystallogical axes, they played an important part in the colour effects of gem stones. The quality of hardness of a stone was of great moment, for upon this depended whether the stone might be employed as an article for personal adornment. Many varieties of minerals would be quite suitable for the purpose in every other respect but this one quality of hardness. And a scale had been devised to indicate the relative degree of hardness.

On this scale the diamond was represented by ten, the sapphire nine, topaz eight, quartz seven, and felspar (moonstone) six. All stones below

the number of five were regarded as too soft for articles of jewellery. In each of these degrees, however, there was considerable variation. In the diamond alone there were very great differences of hardness as between stones coming from different localities, and the same remark applied to sapphires. There was as much difference between the diamond and the sapphire in point of hardness as between the sapphire and the lowest number on the scale. They all had their relative destinies indicated by the relation of size or weight. It was very essential, therefore, for anyone to have an approximate idea of the density—in order to value stones, the individual should be able to gauge to a certain extent the relative weight. One stone of two carats weight might be half the size of another of equal weight.

There were several methods of determining this particular property, for instance, immersion in a heavy liquid of known density, or weighing by a hydrostatic balance. Methyline iodide, unfortunately, rapidly darkened on exposure to light, but was very useful because it did not deteriorate, and was very convenient in use.

Liquids might be diluted down, and then small pieces of mineral kept in them to denote the densities as required. For stones above 3.32 the double nitrites were used. With the hydrostatic balance the weighing might be in air, or water, but in the latter case there was loss by displacement to reckon with. The difference in weight was the amount of the weight of the equal bulk of water the stone displaced, and this divided into its first weight gave the specific gravity. If the weight was 6 grammes in air and 4 in water, the difference was two. The two divided into six gave three, which was the specific gravity of the stone.

Optical Tests for Mounted Stones.—These tests, of course, were only useful in the unmounted state, and in the mounted state optical

tests were applied. The colour of gem stones, although of greatest importance commercially, was of little or no value in determining their particular identity. Certain stones showed characteristic shades of colour, it was true, but it was only after various tests had been applied that they could consider colour any evidence of value at all. Minerals in their pure state were pure white, and the colour shown was due to mineral pigment in many cases. The quantity of colour present in the majority of cases was infinitesimal, because it had been shown that one part of gold would colour 10,000 parts of paste a deep ruby red. It was presumed that colour was due to some pigment, but there could not always be certainty.

The refraction and total reflection of the stone were of great importance in optical tests. Each variety exercised its own particular refraction. Whatever the stone might be the angles of reflection varied and the refractive index was found to bear a constant relation to the angles of total reflection—constant in stones of the same species.

Dr. Herbert Smith, of South Kensington, was responsible for an extremely useful instrument —a refractometer or total-reflectometer. It consisted of a semi-cylindrical lens and glass graduated screen, and the stone to be examined was placed upon the hemisphere of glass and the rays of light reflected by the stone were thrown upon the screen. This created a shadow and was then totally reflected through a brilliant light. The dividing line was read off and the refractive index given. It was particularly useful as it applied even to mounted stones. The great advantage of the instrument was rapidity and applicability to colourless as well as to coloured stones.

Referring to dichroism the lecturer said that refraction was single or double according to the system of crystallisation, and the double refraction was due to irregularity of the crystallised

structure. Some stones had one direction only in which there was no splitting up of the rays, and others had two, giving twin-colour effect. There were monochroic, dichroic, and pleochroic. Colours in the diamond were due to its very high refractive index and high dispersive property, and the effect was seen in distinct flashes of colour. This "fire" was not seen in stones badly or improperly cut. When cut too thick from the table or surface, the angles were not at the right degree to throw this "fire" up to anything like the extent as when the stone was of correct proportions. Again, when cut so as to exhibit too much "spread," the result was poor, and the stone looked too transparent.

Opalescence.—The iridescence of the opal was due to the amount of water in its composition, and the contraction fissures the stone developed during growth. There had been a lot of discussion on these subjects, but they got similar characteristics in other stones where fractures were caused and thin films of air or vacuous films caused diffraction. The opalescence of the moonstone was caused by the enormous number of planes of separation in the stone, light being totally reflected from these planes. "Labradorescence" was due to microscopic planes embedded in the stone—all of unequal length and producing patches of colour in the stone.

"Catseye" was shown by microscopic cavities ranged in parallel groups, and it was estimated there were no less than 200,000 in the space of an inch. Light being reflected from these cavities gave that milky streak in the centre of the stone which, to be accentuated, was cut with high curvature. The same thing occurred in the ruby. Patches of these vacuous canals or cavities gave the effect of "silk" which greatly deteriorated from value and beauty. In the sapphire group sometimes these cavities were ranged in different directions, and there was the peculiar effect of asterism.

ON THE FOCUS OF A LENS.

By J. W. Gordon.

Read before the Optical Society on January 19th, 1910.

At the monthly meeting of your Society, which was held in March, 1909, a paper was read by Mr. Selby, in which the writer was good enough to take notice of a previous paper of mine, and to point out that it had been written without reference to Lommel's paper on the Diffraction Phenomena of a Circular Opening, in the Proceedings of the Royal Bavarian Academy for 1886. Mr. Selby inferred, as indeed the fact was, that I had not seen Lommel's paper, and somewhat summarily dismissed my conclusions as being incompatible with Lommel's results.

Being naturally impressed by Mr. Selby's criticisms, I took the earliest opportunity of reading Lommel's paper, with the result that I have reached a conclusion so very unlike Mr. Selby's that I am prompted to ask the Society to hear me in answer in the hope that I can satisfactorily show that Lommel's paper, far from disposing of the question of the phenomenal focal plane, has in fact no bearing on that question, but does incidentally, by its deficiency in this and in some other respects, show that it is abundantly necessary that the question should be raised and fully discussed.

Let me briefly state the points in issue. It has, so far as I am aware, been assumed without proof, and without so much as a demand for proof, by all writers upon optics, that the visible image formed by a lens is formed in that plane in which the aplanatic rays of light from

a point in the object meet after passing the aperture. The assumption was originally a perfectly natural one. The earliest theoretical writers upon optics dealt exclusively with pencils of aplanatic rays, and gave themselves no concern about the incidental phenomena of diffraction. That state of things, however, has passed away. Nobody who desires to write or speak with accuracy at the present day thinks of dealing with problems of definition or resolution, that is to say, with certain essential problems of image formation, without taking diffraction into account. So soon as we thus enlarge the scope of our enquiry it becomes a wholly unwarrantable assumption that the image which we see is formed in that plane in which pencils of aplanatic rays come to a point. We know perfectly well that the visible image contains a large component of diffracted light; that it is, in fact, a compromise between the punctiform image which aplanatic light might be supposed to produce and the blurred sketch which might be expected to result if all the aplanatic light were suppressed. In performing the operation of focussing we always proceed upon the method of trial and error, and we settle upon that focal adjustment which gives us the best visible result without regard to theoretical considerations of any kind. Now, if it be a fact that the antipoints formed by diffraction are more compressed in a plane lying adjacent to but somewhat behind the aplanatic focal plane, then there is a great probability that the phenomenal focal plane occupies more or less the same position, and is to be sought, not exactly where the aplanatic rays meet, but where the antipoints are most compressed. Whether this is so is the problem which I have ventured to raise for discussion, and my immediate object in seeking the hospitality of the Society this evening is to point out that upon the solution of that problem Lommel's results throw no light whatever. I think, however, that I shall make

no great additional demand upon your time, and shall perhaps add something to the interest of the discussion if I take the opportunity, incidentally, of suggesting those lines of argument which seem to me to point in the direction which I have indicated.

It will, of course, be understood that the interest of this discussion is not directly practical, although it leads to practical conclusions of the very highest importance. Those conclusions are, however, reached indirectly. Upon the basis of the accepted theory as to the focal plane certain calculations have been made touching the necessary and impassable limits of resolving power in optical instruments, and we have even been told, with great show of authority, that those limits have actually been reached by existing instruments. That, if unassailable, is a very momentous proposition, but if those calculations have been made for the wrong plane, then those inferences are wholly untrustworthy, and we may still live in the hope of seeing further improvements, not merely in mechanical details, but also in the optical design of our high power instruments. Thus, although the immediate result of the enquiry can only be a theoretical proposition its ultimate significance is greater than that of any other question at present occupying the attention of the students of optics.

Coming now to Lommel's paper I observe that it is prefaced by an introductory paragraph in which our author sets out the object which he has in view. This, which is expressed with admirable terseness, may best be quoted.

After referring to the work done by Fresnel, Poisson, Schwerd, Airy, Abria, Knockenhauer, and himself (in an earlier paper) in the way of investigating the law of light intensity in the diffraction pattern due to a circular aperture and to a circular screen, he says :—"There thus exist at present on the one hand certain results obtained for the middle of the diffraction image

and, on the other hand, a complete theoretical knowledge of the phenomena in the Fraunhofer case, but independent of one another and without any systematic connection. To fill this gap in the theory and to deduce from the wave theory the whole infinite multiplicity of the phenomena, finally, to test the theoretical results experimentally is the scope of the present paper."

It will be observed that Lommel does not here propose to add to our knowledge—which, indeed, he describes as being already theoretically complete—of Fraunhofer diffraction; that is to say, of the distribution of light in the focal plane. But this is the exact field of the investigation which I propose, and it will accordingly be found that Professor Lommel has, like all his predecessors, taken for granted the very point which I desire to have tested. The simplest way of bringing out this point will be by explaining what he has accomplished, and that may best be done by passing in review the first section of the paper, a section which he devotes to the determination and development of his integral.

It is not very useful to deal in the way of a read paper with mere mathematical formulas. I propose, therefore, instead of following Lommel step by step to lead up to the formula which he adopts in such a way that its significance will be apparent the instant it is disclosed, and no discussion of the formula itself will be necessary. Let me, then, ask you to assume that we are interested in the solution of a problem different in detail from Lommel's, but presenting to a large extent the same subject matter of calculation. Imagine that you are standing beneath a dome, as it might be, say, the dome of St. Paul's, but assume that it is exactly hemispherical in form and constructed entirely of ground glass. Assume, further, that it is lighted not by direct sunlight, but by the diffused light from a bright but cloudy sky, and that the problem is to ascertain how much light such a dome would send to the observer's eye.

An obvious answer suggests itself. Two things will have to be considered, (1) the area of the dome, (2) the brightness of its illumination.

Now, the second of these may for present purposes be at once dismissed from consideration. Of course, the amount of light will be greater at noonday than at twilight, but that variation is easily allowed for. We simply have to multiply by some factor representing the intensity of the external light the expression, whatever it is, which we arrive at by calculating the area of the dome. Since this simple arithmetical operation can be performed at any time, we will not bother further about it this evening.

There is left the area of the dome to be considered, and this problem is complicated by the consideration that all the light which reaches the spectator must reach him through the circular opening at the bottom of the dome. You cannot increase the amount of light which the dome will yield by merely augmenting its area so long as it is seen by projection through a limited aperture. You must therefore be content to go warily. It cannot, however, be wrong to assume the dome to be divided up into windows, and having calculated what amount of light each window will transmit to take the sum of the light so calculated for all the windows as being the illumination due to the dome.

A Window Pane Analogy.

There is an obvious way of arranging windows in such a structure. Those members of the frame which I may call the vertical members would naturally converge to a point in the vertex of the dome, whereas the other parts of the frame would be arranged as rings, probably equally distant from one another; thus dividing the whole area up into a multitude of trapezoidal panes. An expression for the dimensions of any one of these planes is easily found. Let us write (a) for the radius of the dome, and taking one of the panes in the lowest row, that is to

say, those skirting the edge of the dome, for our unit let us assign (f) for the breadth of the frame, and (p) for the height of the frame, then obviously, the area of that pane will be ($f \times p$). Taking that for datum, if we select any other pane anywhere in the dome, and if we write ψ for the angle between the vertex of the dome and this selected pane; its polar distance, as astronomers would say; then the dimensions of this pane will be given by the following expressions. Its vertical height will, as in the other case, $= p$, its breadth will be ($\sin \psi f$). Now, the amount of light which will enter through these two panes will be proportional to these two areas respectively. But it does not follow that the amount of light which reaches an observer on the floor of the building will be similarly proportional to those two areas. On the contrary, it is pretty certain that the lower pane would be more foreshortened than the upper, and consequently the area of its projection or, what I may call, its aperture value, will be less than its real area to an extent determined by this foreshortening. Without for the moment going into that matter, we may take it for granted that the measure of that foreshortening can be expressed as the cosine of a certain angle, which we may write down ($\cos z$) and, therefore, for the amount of light to be radiated to a given point upon the floor from any window in the dome we must write ($\sin \psi \, f \cos z \, p$) and for the sum of all the panes which we may therefore write,

$$\frac{2 \pi a}{f} \sum_{\psi = (o \quad z_1)}^{\psi = (\pi/_2 \, z_2)} \left(\text{Sin } \psi \, f \cos z \, p \right)$$

an expression which will give us the amount of light radiated in the direction of our spectator by the dome.

There is, however, another consideration which must be taken into account. Although the light radiated in the direction of the spectator

has an initial value represented by the above expression, its value when it reaches him will depend partly upon his distance from the dome, that is to say, the farther off he is the smaller will be the proportion of the total light which the dome transmits that will reach his eye. It is well known that the falling off of light intensity due to this cause varies inversely with the square of the distance, and if we take the light transmitted to a distance equal to the radius of the dome to be that represented by unity, and if, moreover, we write (d) for the distance between any given pane and the spectator, then the amount of light which would reach our spectator would be measured by

$$\frac{a^2}{d^2}\left(\text{Sin } \psi \, f \, \text{Cos } z \, p\right)$$ and the total light will be

$$\frac{2\pi a^2}{f}\sum_{(o \quad z_1)}^{(\pi/2 \, z_2)}\left(\frac{1}{d^2}\text{ Sin } \psi \, f \cos z \, p\right)$$

So far our problem has been analogous to that with which Lommel is concerned, because down to this point our glass dome has not differed in any material respect from a wave front. But here we must observe a distinction. A dome lighted in the way here suggested would give us a steady average illumination, in which there would be no distinguishable phase. A wave front, on the contrary, if it coincided with the dome, would give out light constantly varying in character, and varying so much that it would be alternately positive and negative light. We must therefore modify the conditions of our problem to correspond, and we may find a sufficiently close analogy by the help of those electric signs which have become so formidable a feature of the modern street. Everybody is familiar with those unpleasant advertisements in which changing colours chase one another through the lettering of some uninteresting legend. Now let me suppose that our dome is lighted by elec-

tric lamps which change colour in this way, and that the scheme of lighting may be described thus:—Assume two colours, say, for example, blue and yellow, which are to alternate and chase one another in radiating zones from the vertex towards the edge of the dome. If, under these conditions, you wanted to know what was the colour of the compound light that at any moment would be radiated from your dome upon the spectator, you would have to consider not only the area of every pane but also the particular colour of the light which at a given moment it was radiating. You would require, therefore, to go over the foregoing calculation once more, supplying a co-efficient which would tell you whether at the given instant your electric lamp was radiating blue light or yellow light. Consider, then, what form this factor would take. All the coloured zones start from the vertex, and if we assume that blue and yellow follow one another in true periodic succession, we can, by a very simple calculation, tell what at any moment is the colour of the light being radiated from the *vertex* of the dome. Let us assume that the colours change once in some interval of time which we may express by writing it $=T$. Now, suppose that we know the exact time at which the illumination began and that, measured from that moment, the time at which our observation is made $=t$, then t/T will give us the number of alternations in the colour at the vertex of the dome. We may, therefore, write $t/T=c$ and then, according as c is odd or even, the colour will be blue or yellow. That tells us all about the coloration at the vertex of the dome, but if we want to know the colour of a pane which is situated elsewhere in the dome we must, obviously, make allowance for the time which the colour takes to travel from the vertex to that selected pane. This allowance is easily arranged for. We may write λ = the distance that the colour traverses in the time T, and if we write $\triangle$ for the distance, measured along

the vertical member of the frame, at which our selected pane stands from the vertex, then $\triangle/\lambda$ will indicate the extent to which this pane is behind the vertex in the matter of colour. Thus we obtain the following expression for the colour of the given pane

$$c_1 = \left(\frac{t}{T} - \frac{\Delta}{\lambda}\right)$$

and according as (c_1) is odd or even so the colour of the given pane will be blue or yellow. The expression which we have so far found would give us exactly what we want, provided the expression within the bracket always gave us an integral number. But obviously this is a condition which will not, in fact, be satisfied, and therefore we shall have values of (c_1) which are neither odd nor even but, on the contrary, are very unhandy fractional quantities not to be classified in the required way. We need, therefore, to seek for some more suitable expression for the colour, and there is a well-known mathematical formula which satisfies the case.

If we give to the magnitude $\left(\frac{t}{T} - \frac{\Delta}{\lambda}\right)$ an angular value, we shall have a series of magnitudes which will be alternately positive and negative. Thus $\sin\left\{\left(\frac{t}{T} - \frac{\Delta}{\lambda}\right) 2\pi\right\}$ is a cyclical expression which will exactly meet our requirements. It rises to a maximum, comes down to zero, proceeds to a minimum, returns to zero, goes to the maximum again, and is alternately positive and negative, always between the limits $+1$ and -1. We may therefore adopt this as a criterion for distinguishing between our blue and our yellow light, and if we assume that our blue and yellow lights are complementary, and will exactly cancel one another when they are present in equal force, yielding then a colourless

light, we shall be able to obtain by means of this expression a quantitative determination of the extent to which the blue and yellow lights will enter into the illumination of the floor. Combining this result with that already obtained, we get the following expression :—

$$\text{Light} = \frac{2\pi a^2}{f} \sum_{(o \quad z_1)}^{(\pi/2 \; z_2)} \left\{ \frac{1}{d^2} \text{Sin} \left(\frac{t}{T} - \frac{\Delta}{\lambda} \right) 2\pi \right.$$

$$\left. \text{Sin } \psi \, f \cos z \, p \right\} \quad \ldots \quad \ldots \quad (1)$$

A somewhat simpler expression may be obtained if, instead of the full light intensity, which is the resultant of an indefinite number of displacements in one plane, we measure one of those displacements only. We then get an expression involving only first powers instead of the squares a^2 and d^2. Still, however, an expression in the form above stated represents the light radiated by a polygonal surface. If we want to pass from this to a spherical surface we must, of course, assume that each pane is divided up into infinitesimally small panes, and that in the process the opaque frame is obliterated. We may then, using the notation of the differential calculus, write $f = d\phi$; $p = d\psi$. In these expressions ϕ is, of course, the angle of azimuth, and ψ, as already defined, the polar distance of the given pane. Substituting these values for f and p in the above expression, and making use of the notation of the calculus, we obtain the following expression for the light which would be received from a hemisphere of ground glass under the conditions specified, the light being measured by the amplitude of its displacement at a given point.

$$\text{Amplitude} = a \int_{o}^{2\pi} \int_{(o \quad z_1)}^{(\pi/2 \; z_2)} \frac{1}{d} \text{Sin}$$

$$\left(\frac{t}{\tau} - \frac{\Delta}{\lambda} \right) 2\pi \text{ Sin } \psi \, d\phi \, \text{Cos } z \, d\psi \quad \ldots \quad (2)$$

It will be observed that the operation denoted by $\int_0^{2\pi} \text{Sin}\ \psi\ d\ \phi$ in this expression is equivalent to that denoted by $2\pi a/f \sin\ \psi\ f$ of (1). Bearing this in mind, and substituting the amplitude factor a/d for the intensity factor a^2/d^2, the reader will perceive that equation (2) is directly deducible in the way indicated from equation (1). It may be useful to point out further—in elucidation of this expression—that the angle z is a function of ϕ and ψ, the exact form of which is of no importance in the present discussion, and that the quantity $\triangle$, as already indicated, is equal to $a\ \psi$.

If now we compare this expression with the expression which Professor Lommel starts to integrate, we shall see at a glance what is the scope of his undertaking. He writes under his first Section, which bears the headline, "The determination and development of the integral" :—

"The disturbance produced at the point $\epsilon\ \eta$ of the screen by any given part of the spherical wave is then expressed by the double integral

$$a \iint \frac{1}{d}\ \text{Sin}\ 2\pi \left(\frac{t}{\tau} - \frac{d}{\lambda}\right)\ \text{Sin}\ \psi\ d\ \phi\ d\ \psi$$

when the integration is extended to all effective parts of the spherical wave. . . . .

"(2) In the case of a circular opening having its centre upon the polar axis, and lying in a plane parallel to that of the screen the effective part of the spherical wave is defined by the boundary of the circular opening, and the amplitude of vibration at the point $\epsilon\eta$ is expressed by

$$a \int_0^{2\pi} \int_0^{\psi_1} \frac{1}{d}\ \text{Sin}\ 2\pi \left(\frac{t}{\tau} - \frac{d}{\lambda}\right) \text{Sin}\ \psi\ d\ \phi\ d\ \psi \qquad \ldots \qquad (3)$$

where ψ is the angle which the ray passing from

the edge of the opening to the radiant point makes with the axis." The rest of the paper is taken up with the integration of this expression.

Now, understanding that the same symbols represent the same factors in equations (2) and (3), and comparing this last with the expression which we just obtained, it will be noticed that there are three points of difference. First of all, in place of $\triangle$ Lommel uses the distance d; next he substitutes for $\cos z$ the value unity, and in the third place he makes the integration in respect of the angle ψ to a limit indicated by ψ_1, and not, as in our expression, over the entire quadrant. What is the significance of these changes?

First, as to the substitution of d for $\triangle$. This can be very simply explained. $\triangle$, in our expression, is the distance which any coloured ring has travelled at the moment of observation from the vertex of the hemisphere. That distance it is which determines the colour of the light at the given moment. That distance, however, has no significance for Professor Lommel. He is not concerned with changes of colour, but with variations of phase, and the phase does not vary at any given instant over the surface of his wave front. He therefore is able to write $\triangle = 0$, and to eliminate this from his integration. On the other hand the difference of phase with which rays from different points upon his wave front arrive at the point where the amplitude is to be calculated is determined by differences in the distance d, that is to say, the variable distance of a point on the wave front from the point of observation, or to recur to our original illustration, the various distances of different parts of the dome from the spectator on the floor. Consequently, this spectator distance d must, in Lommel's formula, take the place in our formula of the polar distance $\triangle$. The change does not in any way affect the nature of the calculation, but only the calculated result.

Next, to take the difference of the limits over which the integration is to go. Professor Lommel does, in fact, when he comes to deal with the problem of assigning a value to his integral, limit himself expressly to very small values of the angle ψ_1. This simplifies the problem, perhaps it would be better to say, brings it within the compass of calculation. It is very important to realise how small this angle is. Professor Lommel stipulates that it shall be taken so small that the sine and arc may be substituted interchangeably for one another (page 237), that is to say, Professor Lommel is, in substance, dealing with a plane wave front. This is one of the reasons why I said at the outset of this paper that it does not appear to me that Professor Lommel's results have any bearing on the question which I have raised. The problem of the position of the focal plane can only arise when we are dealing with beams of light having a considerable divergence angle. And here, perhaps, I may interject the observation that in the case of the microscope—with which I am chiefly concerned—those angles are very considerable indeed.

The third point of difference is the substitution of the value cos o for cos z. This is legitimate on the assumption that the point of observation is taken not very far from the centre of the sphere of which the wave front forms a segment. My impression, as the result of a somewhat cursory reading of Lommel's paper, is that he does not expressly lay down this condition, but, whether or no, he does in fact observe it. His calculations are not made for distances extending to very many wave lengths from the focal point and hence, within the limits which he practically observes, it is quite legitimate to treat this angle z as having a zero value.

We now see the scope of Professor Lommel's undertaking, so far as he deals with calculated results. He is dealing with a wave front of infinitesimally small angular magnitude, and,

considering the diffraction pattern in a region which is either the focal point for aplanatic rays or a region lying closely adjacent to that point. It follows that, so far as calculation is concerned, he does not touch the problem of the phenomenal focal plane for strongly convergent beams of light. He adds to his paper certain experimental results, and speaks of these as strikingly confirmatory of the results to which his calculations lead. From what has been already said, the Fellows of the Society will be prepared to understand that Professor Lommel does not in fact deal with beams of focussed light. His observations were made upon direct sunlight. The apparatus with which he worked is thus described on page 298.

Lommel's Apparatus.

"In order to compute the phenomena theoretically, and so be able to compare them with the results of observation, it is necessary to make use of homogeneous light of a definite wave length. There was produced by means of a clockwork heliostat, slit, achromatic lens, and flint glass prism, a clean sharp solar spectrum, and this was thrown upon a screen furnished with a small round aperture $\frac{1}{4}$ mm. in diameter. This small aperture was advanced to the several Fraunhofer lines by moving the screen in a straight line until the dark line occupied the middle of the aperture, and thus light in the immediate neighbourhood of the dark line on either side of it passed through the aperture. This little aperture thus served as a nearly punctiform source of homogeneous light for the diffraction apparatus which was situated at a suitable distance behind it.

"In the whole research the distance ($a = 2120$ mm.) of the light source from the diffraction opening was retained unaltered, and only the distance, b, between the opening and the micrometer varied.

"Moreover, the diffracting opening was the

same in all the observations. Its diameter, measured by means of a glass micrometer and a microscope, came out 0.56 mm. Thus its radius was 0.28 mm.

"The measurements were taken over the Fraunhofer lines, C, D, E and F. For the lines B and G the phenomena were not sufficiently bright for secure measurements."

It will thus be seen that the light which suffers diffraction is only the narrow pencil which, after passing the first aperture, $\frac{1}{2}$ mm. in diameter, is transmitted at a distance of about seven feet by another aperture of substantially the same size. Such a beam of light would consist, like Lommel's calculated beam, of substantially plane wave fronts.

It thus appears that both the calculated and the observed results are very remote from the question of diffraction phenomena produced by strongly convergent beams of light and, speaking for myself, with all the will in the world to derive enlightenment upon the question under discussion from Professor Lommel's paper, I must confess that I have been unable to extract from it so much as a single grain of relevant information.

The New Line of Argument.

I hope, however, that I shall not be thought pertinacious if I venture to suggest, for the consideration of the Optical Society, one of those lines of argument which seem to me to point irresistibly to the conclusion that the minimum extent of the false disc must lie at a position slightly beyond the aplanatic focal plane. I confine myself to a single line of argument because a complete discussion of the matter would involve something like a small treatise and, having regard to the necessary limits of space and time, I select that line of argument which appears to me most easily compressible into a small compass. In a paper read before the Royal Microscopical Society, in December, 1904,

which it would be tedious here to recapitulate, I pointed out that all the light which reaches the focal plane outside the focal point must pass through the conical surface which forms the boundary of a focussed beam of light. Now, we know just as much about the condition of that boundary as we do about the condition of the wave fronts which are contained within it, and therefore it must be just as legitimate to start our integration over that surface as over the

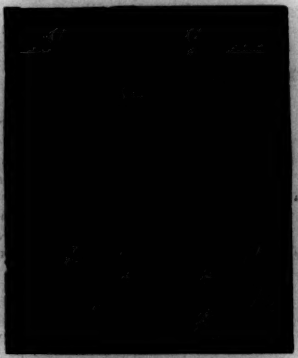

FIG. 1.

surface of the wave fronts. I endeavoured to perform the necessary operation, and although I did not succeed in obtaining an expression from which I was able to get a numerical result, certain interesting conclusions of a general character at once presented themselves as the outcome of investigating the matter along those lines. The following diagram (fig. 1) will serve to illustrate the conclusion reached.

Consider under what conditions light dif-

fracted from the surface of the convergent beam $a\,a_1\,c$ can reach the point p_1 upon the focal plane. Draw $p\ .\ .\ .\ p_1$ perpendicular to the edge $a_1\ .\ .\ c$. It is at once obvious that no light of any kind can be radiated upon the point P_1 from any part of the conical surface which lies below the level of point P, for all the wave fronts below that point would have to radiate backward any light which they might radiate to the point P_1. We may therefore look upon the horizontal plane through the point P as being the lower limit of the surface of the focussed beam which radiates diffracted light upon the point p_1. Another conclusion easily established is that we can find a point m such that the light reaching the point p_1 from that point and from the whole area above it on the surface of the focussed beam will be negligibly small by reason of the fact that light from the successive zones upon this part of the surface of the beam interferes so as almost to produce darkness, and that the small residual illumination comes from a surface subject to strong foreshortening, and consequently having very small aperture value. We thus get a definite area consisting of a series of zones, not, in fact, a very extensive series, upon the surface of the beam from which alone light can effectively be radiated to the point p_1, and it is to be observed that there are both upper and lower limits to the region so defined.

Now, the point p_1 is, in the diagram, shown as a point in what I have called the focal plane for aplanatic rays, and it may quite well be assumed to be the dark ring which forms the boundary of the false disc. It is obvious from the diagram that if we selected a point p_2 below that focal plane we could define an area, marked out by dotted lines in the diagram, upon the surface of the cone, which would stand in the same geometrical relation to the point p_2 as that in which the radiant surface $m\ .\ .\ p_1$ stands to the point p_1. General considerations, which do not

involve the discussion of the diagram at all, show that if the point p_1 in the focal plane for aplanatic rays represents a ring of zero illumination the point p_2 must similarly represent a ring of zero illumination in the sub-focal plane in which it lies. If we now select another point in the position marked p_3 in the diagram, the distance of which below the focal plane for aplanatic rays is equal to $\frac{1}{2}$ sin 2 u r; 2 u being the angular aperture of the beam, and r being the radius of the ring defined by the point p_1 in the aplanatic focal plane; in that case, we should find a dark ring forming the boundary of a false disc, which would receive the entire radiation from the whole beam of light, which dark ring would pass through the point p_2, and it is clear from the diagram that its axial distance is dimensioned to the axial distance of the point p_1 in the proportion $\cos^2 u$:1

This conclusion does not stand alone. It is possible to carry the point p_1 above the aplanatic focal plane as well as below it, and in that case we still obtain a ring, now of increasing dimensions but of zero illumination, and thus arrive theoretically at the conclusion that the false disc above the aplanatic focal plane must open out into a luminous ring bounded as before by a circle of zero illumination at its outer edge, but of increasing dimensions in accordance with the increased sectional diameter of the beam itself.

Now, this theoretical result is obviously one that can be very easily verified. Any system that will show the boundary of the false disc at all must be capable of showing its expansion into a bright ring surrounding the lower end of the focussed beam of light, and the expansion of this ring may be made to take place upon so obvious a scale that there can be no instrumental difficulty in observing it. Having reached this result as a theoretical conclusion I proceeded to test it experimentally. In fig. 2 I am able to present to the Society a photo-

graphic reproduction of a series of sections of a focussed beam of light, the sections being obtained by focussing the camera upon successive

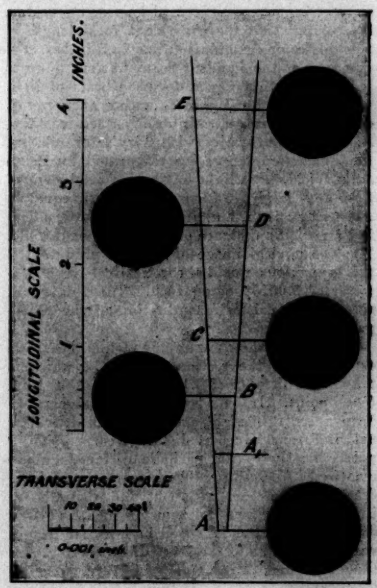

FIG. 2.

planes in the course of the beam. The visibly focussed image is shown at A, being the image of an artificial star having a circular aperture of 1/100 of an inch in diameter, illuminated

from behind by the light of a Welsbach mantle. This artificial star was placed at a distance of 187 inches in front of a telescopic combination of a telephoto lens, with a well-corrected photographic lens and the field lens of an ocular. By computation the equivalent focal length of this telescopic combination was 25.6 inches. The aperture of the incident beam was cut down to $\frac{1}{4}$ inch. In this way a focussed beam of light was formed, having an angular aperture of about twenty-seven minutes. Such a beam should in conformity with Airy's formula for the diffraction fringe, as elaborated by Helmholtz, yield, with light having a wave length of 1/50,000, a fringe having a breadth equal to .0024 of an inch, and a diffraction pattern having a periodic distance of twice that magnitude. The geometrical image of the star produced by this apparatus would theoretically have a diameter of 0.00159 of an inch. These data will suffice for a critical examination of the photograph.

Proceeding downwards along the beam we come first to the section marked E, which is taken at a distance of 5.1 inches from the visual focal point. It will be observed that here we have a central bright spot surrounded by two bright Fresnel rings at distances corresponding pretty nearly to the periodic distances above mentioned. Measured by the scale, the periodic distance comes out at about 3.6 thousandths instead of 4.8 thousandths of an inch. This we should expect. For it is well known that images vary directly as to the focal distances at which they are formed from the apertures forming them, and it results from Helmholtz's investigation that diffraction patterns are subject to the same law of relative magnitudes as images. Now this diffraction pattern in the plane at E is formed at about $20\frac{1}{2}$ inches from the aperture, whereas the periodic distance 0.0043 in. is calculated for a focal distance of 25.6 in. We should therefore expect to find the scale of the

periodic structure reduced in this plane to about 4-5ths of its value in the plane for which the value 0.0048 in. was calculated. We find it reduced in ratio of 36 to 48. This is a coincidence which must to some extent be lucky, for it is, assuredly, well within the limit of the experimental error.

Errors of Centration

It will, no doubt, be remarked that the beam, in this section of it, shows as being not quite symmetrical. The explanation of that is obscure, but I surmise that it may be found probably in some want of accurate centering of my lenses. From the description above given of the apparatus with which these photographs were produced it will be seen that several lenses separately mounted had to be employed, and as, in fact, my optical bench was of a very rudimentary character, I had great difficulty with the centreing.

Coming next to the section at D (distant 3.7 inches from the visual focus) it will be observed that the second bright ring, counting from the centre, seems to have disappeared, the central spot and first ring remaining in place and the outermost ring having contracted into the space vacated by the second. What, in fact, has happened, however, is that all the rings have contracted equally. The central bright spot of E has disappeared altogether; the first bright ring of E has condensed into the central bright spot of D; the second bright ring of E has contracted into the first bright ring of D, and the outermost ring of all has contracted in the manner which is evident upon the face of the photograph. All these contractions have proceeded quite gradually in the space from E to D, and the reason why in the lower plane the central pattern of the upper plane is so exactly reproduced is that the distance from E to D is a periodic distance measured along the axis of the beam.

Passing over another periodic distance we

come to the plane C (distant 2.3 inches from the visual focus) where we find the typical pattern of E and D again reproduced *mutatis mutandis*. In view of what has been already said it would be superfluous to discuss this section in detail, but it is not superfluous perhaps to point out how precisely the periodic distance C . . . D is equal to the periodic distance of D . . . E.

The next photograph was taken at a shorter interval than the periodic distance, and was, in fact, produced with the object of obtaining a section showing the black central dot. This, of course, results from the condensation of one of the dark rings, and it will easily be understood that the dark dot in B results, in fact, from the condensation of the dark ring in C. It would of course be expected, and rightly expected, that this black dot would be formed at half a periodic distance from the bright dot, and, in fact, the plane B does occupy that position. The black dot, however, does not make so good a figure as the white dot, for the simple reason that it has been eaten into by halation.

Analysis of the Outer Ring.

We are now brought face to face with a question which down to this point it has been possible to evade, namely, what is the constitution of the outermost ring? According to the systematic arrangement of Fresnel rings we should expect to find a Fresnel ring surrounding this black dot of the B section, but the ring which actually surrounds it is disproportionately large. If the aperture here contained only one fully developed dot and one ring it would have diameter of a period and a half, that is to say, of about a little over 7/1000 of an inch. In fact, however, the mean diameter of the B section is $10\frac{1}{2}$/1000 so that we obviously have something more than a Fresnel ring surrounding the black dot. Independently of this consideration we know that there must be something in addi-

tion to the Fresnel ring, that is to say, there must be a diffraction ring encircling the beam and perhaps, therefore, we may take it that what we see in section B is a mutilated black dot, an indistinguishable Fresnel ring, and surrounding the whole a diffraction ring. If the outermost Fresnel ring and the diffraction ring could be considered to be entirely independent of one another we should infer that the breadth of the beam at this point ought to be two and a half times the periodic length of the diffraction system. The long diameter of the elipse itself satisfies this condition almost exactly. The shorter diameter, however, falls appreciably short of it. Speaking for myself I find it difficult to offer any observations upon this measurement, for I am not sufficiently informed as to the connection between the outermost Fresnel ring and the circumjacent diffraction ring, to know whether the two adjacent edges will overlap, and to form any opinion, therefore, as to whether the longer or the lesser axis more truly represents the actual state of things in section B.

The Visual Focal Point.

The next section, of which I have a photograph, is at the visual focal point A. I may here be allowed to explain that when these photographs were taken I had no notion about the periodic distance in length, and no means, therefore, of identifying the theoretical focal plane for aplanatic rays. In fact, I had not perceived, until I came to put these photographs together systematically for the purpose of illustrating this paper, that there was any means of ascertaining precisely where that plane is to be found. When, however, these photographs are assembled as they are in fig. 2 it seems pretty obvious that the focal plane for aplanatic rays must lie at a point about midway between A and B, which point would lie at the periodic distance from the plane C and would, in fact, be the focal plane for the outermost of the Fresnel

rings. Unfortunately, through inadvertence at the time I failed to obtain a photograph of this most interesting section. But it will, nevertheless, be of interest to consider its dimensions as indicated by the diagram. We know exactly what the calculated dimensions of an image formed in this focal plane for aplanatic rays would be. The geometrical image of the artificial star would have a diameter of .0016 of an inch. The Airy diffraction ring would have a breadth of .00237 of an inch. Taking this last mentioned dimension twice and adding the breadth of the geometrical image we get .0063 for the calculated diameter of the image of the star. Applying the scale at the plane marked A_1 we find the diameter to be 0.008. It will be borne in mind that the diagram indicates the greater axis of the elipse and that in the plane B the two axes differ by 1/500 of an inch. If we assume a difference of half this amount between the two axes in the plane A_1 we should then have 0.0075 for the mean diameter of the image formed here. Having regard to the imperfect corrections of the system and to the somewhat elaborate calculations by which these numerical results have been reached, I think it may be taken that that is as close an approximation of the experimental to the theoretical results as could at all be looked for.

If now I have successfully identified the focal plane for aplanatic rays it follows that the visual image is in fact formed in that plane in which the diffraction ring condenses upon itself and not in the theoretical focal plane. If this conclusion is sound it follows also that all the calculations as to the *minimum visible* and the ultimate limit of resolving power hitherto made must be revised. Those even which have been calculated for beams of very small divergence angle, probably exaggerate by something like 50 per cent. to 100 per cent., the difficulty of rendering small objects visible. Those calculations, or, rather, inferences, which have been

made concerning the resolving power of beams of wide angle will have to go by the board.

Mr. Selby, in opening the discussion, said it was a matter for self-congratulation to him that he was in some degree responsible for Mr. Gordon's appearance before them that evening, in that on a previous occasion he (Mr. Selby) had ventured to criticise Mr. Gordon's conclusions. He regretted, however, Mr. Gordon should have thought it necessary to devote so much attention to the matter he (Mr. Selby) had then brought forward. That matter was in itself intrinsically interesting, as were all Lommel's results; but there were so many other points on which those who were interested would have liked some further information from Mr. Gordon, the question as to what, in the first instance, led him to his conclusions, whether his reasons were theoretical or experimental; the question as to the exact position he would assign to the focus of a lens; in what respects he regarded the ordinary theory as at fault—in short, what he would have desired was a full and complete statement of Mr. Gordon's views with regard to the focus of a lens, not merely a reply to the criticisms based on the conclusions of Lommel. But since Mr. Gordon had devoted so much attention to those results of Lommel it was necessary, to a certain extent, to follow him, and to say a few words in defence of the position he (Mr. Selby) had taken up. He would like, in the first place, to read a few extracts taken from Lommel's paper. It was not possible to follow Mr. Gordon through all the details of his calculations. The substance of his remarks was that Lommel's results only applied to a plane wave, or only to a portion of a spherical wave, so small that, for all practical purposes, it must be regarded as plane. (Mr. Selby then read the extracts referred to.) If he had fallen into error in supposing that Lommel was speaking of a spherical wave, and not a plane wave, it was clear from these extracts, that Lommel himself had also

fallen into that error. Lommel's results were a very close approximation for an angle up to 10° on either side of the axis of the lens, that is, an angle of 20° altogether, a considerable angular aperture. Mr. Gordon had further remarked that Lommel's results did not apply at all in the case of an image formed by a microscope objective, where, he said, the angle was very much greater than would come under Lommel's formula. That statement showed some confusion of ideas. The case chosen was preeminently one in which Lommel's results did apply, the angle of the image forming pencil in the microscope being quite small. The formula did not attempt to take account of aberration in the lens, nor were they concerned with aberrations on the definition of the focus. If, then, the microscope lens were free from aberration, giving an aplanatic image of a point, Lommel's results did very accurately apply. Mr. Selby proceeded further to discuss Mr. Gordon's reasoning based on the consideration of the secondary wavelets from a corneal surface in place of the spherical wavefront, and pointed out that this procedure was beset with considerable difficulties, and needed much justification, which had not been attempted. Attention was drawn also to the misleading character of the figure from which Mr. Gordon's conclusions were derived. If this figure were received with due regard to the relative dimensions of the lens and the first dark ring in the image it would appear that Mr. Gordon was treating the major portion of lens as inoperative.

Comments by Dr. Clay.

Dr. Clay said he desired, with Mr. Selby, to say how much they had all been interested in what Mr. Gordon had put before them. The subject was one of theoretical interest only, as it would not in practice very much matter whether the focal point was to be removed a small distance from the position usually assigned

to it. He would like to emphasise what Mr. Selby had said about the diffraction cone in the case of the micro-objective.

He did not quite follow why Mr. Gordon wished to substitute a much more complicated method of obtaining the distribution of light at the focal point by considering the light from the whole of the cone, for the usual method of compounding the light from the wave surface where it issued from the diaphragm. If Mr. Gordon was right in his contention that to obtain the resultant focus one must deal with the *volume* distribution of light in the case of a formation of an image by a lens, it would be equally true in any case of interference; for instance, in the simple interference of light that obtains with a bi-prism, the light would have to be added up from the whole volume; but in this case, as everyone knows, the pattern obtained agrees perfectly with that which should theoretically be obtained by dealing with the light from the two points only.

As regards Mr. Gordon's photographs, Dr. Clay thought the results were not at variance with the ordinarily accepted theory. It seemed to him that he was really dealing with a very similar problem to that of the diffraction of light through a circular aperture. It was well known that in this case if the beam of light which issues from the aperture is examined by an eye-piece, and that eye-piece is slowly brought from a distance towards the aperture, a series of points are found at which there will be (for any one colour) a bright spot in the centre of the field. The positions of this spot are approximately those at which the aperture will contain an odd number of Huyghen's zones. It would be found that the distances from bright spot to bright spot were not equal to one another, but that they get less as the eye-piece approaches the aperture. This was exactly what Mr. Gordon had found in his experiment. Dr. Clay thought that if Mr. Gordon would work out the

positions in which the cone of light as it issued from the diaphragm in this experiment contained an odd number of Huyghen's zones, he would find that the positions agreed with those obtained experimentally.

Experimental Tests.

Mr. T. Smith (Secretary) said he had been very much interested in what they had heard from Mr. Gordon and from the other two speakers. There were one or two things that he had wanted to say, but he was afraid, as the hour was getting so late, that he must curtail his remarks as much as possible. In the first place he very much regretted that Mr. Chalmers had been prevented from attending through indisposition. He understood that Mr. Chalmers had hoped to bring some apparatus down and give a little demonstration by which he had hoped to convert Mr. Gordon himself to the usually accepted theory. He (Mr. Smith) thought that the problem by which Mr. Gordon had illustrated the integral obtained by Lommel was not precisely analogous to the actual problem under discussion. For instance, Mr. Gordon had assumed that different portions of the wave front, as represented by the outline of the dome, were in different phases at any particular instant of time, whereas in the actual wave front of light the phase at a particular instant was the same everywhere. Mr. Gordon suggested that Lommel's results were vitiated because higher terms than the second in cosine z were neglected. If they considered Mr. Gordon's illustration they would see that the chief effect due to obliquity was equivalent to a reduction of the aperture of the dome, and thus there was nothing in Mr. Gordon's analogy to bear out the objection. He (Mr. Smith) considered that Mr. Gordon had not made out his case against Lommel, whose results were quite in agreement with experimental work carried out at the National Physical Laboratory.

Mr. Gordon, in replying, said:—He first wished to thank them very much for the kindly welcome they had extended to him, and to say how very sensible he was of the great kindness with which he had been treated by Mr. Selby, and also by other gentlemen present, like Dr. Clay, who felt themselves interested in maintaining the accepted theory of the focal plane. He also felt how much the meeting had suffered by the absence of Mr. Chalmers, whom he, for his own part, had greatly hoped to see there. He would not venture to occupy more than a very few minutes, and must therefore leave many parts of the question undiscussed.

He was sure, for instance, that Mr. Selby would appreciate that it was for that reason he begged to be excused from answering his question as to where exactly the focal plane should be defined. To deal with that would occupy more time than they would care for him to take up, and more time than he was capable of usefully taking up just then. Similarly, with regard to the quotations which Mr. Selby had made from Lommel's paper, he would just say one word on that matter. He did not mean to suggest at all that Lommel did not range beyond the plane wave front, nor did he mean to suggest that Lommel's result applied only to what could be strictly called a plane wave front—in fact, he thought he had guarded himself against slipping into that error, but probably he did slip into it notwithstanding his desire to avoid it.

Case of the Telescope Objective.

He had, however, supplied the correction to anything of that sort by saying that Lommel's calculations were found to apply with remarkable accuracy to the case of the ordinary telescope objective; and that had been used against him by both Mr. Selby and Dr. Clay, because the microscope objective had a still smaller angle behind the objective. The telescope objective

had very often as big an angle as 10°, whereas the microscope never had a backward angle of any sensibly greater magnitude. That was perfectly true but there was a distinction between the two things, and notwithstanding what both Mr. Selby and Dr. Clay had said, he must claim the liberty to maintain against them both that the important distinction was, that the front angle in the microscope objective was very wide. Mr. Selby had dealt with that, and he, Mr. Gordon, understood that he commanded Dr. Clay's suffrage in doing so, but Mr. Selby dealt with that in this way. He said it was only the backward angle which had to be considered, that the forward angle did not matter, because the light from that impinged upon the lens and was not emergent from it. He confessed he (the speaker) should have thought so too, but for a paper which started him upon the whole of that enquiry—Helmholtz's paper, written in 1874, on the question of the resolving power of the microscope. Helmholtz there demonstrated a very remarkable proposition having a direct bearing upon that point, which was the answer to what Mr. Selby had said. The proposition was that in considering the case of the microscope, and the diffraction of the microscope, they need not bother about the diffraction which occurred behind the objective because the diffraction was the same in all parts of the microscope, that is to say, the diffraction was produced by the smallest aperture, wherever that aperture was. Usually the smallest aperture in the microscope was the image of the light source, and therefore the diffraction produced by the microscope usually resulted from the peculiar shape and the position of the image of the light source. If they threw the image of the light source actually into the focal plane, so that its repeated image fell in the focus of the instrument, then it gave rise to no diffraction. But if the image of the light source fell anywhere else in the microscope, it invariably set up diffraction,

because in that case it was always the smallest aperture in the instrument. That aperture was met with in several points of the instrument. They met with it either in the image plane or somewhere near the image plane, and they met with it again behind the ocular. And the main point, and the great point, which Helmholtz demonstrated in that paper was that diffraction could be calculated from that aperture wherever it was met with—it was always the same. The diffraction was the same if the diffracting aperture was found in the Ramsden disc, where they had a plane wave front, or if they found it in the principal focal plane behind the objective where they had a very deeply convex wave front; and, so far as the practical calculation of diffraction from curved wave fronts was concerned that problem, although it was by the Airy and Lommel method, as he thought they were all agreed, an insoluble problem, that is to say, it could not be solved by the process of integration, yet by Helmholtz's expedient the calculation could quite easily be made. Coming back to Lommel's paper, the limit of Lommel's results, which were the same as those of Airy, did not extend beyond something like 10° of divergence and of 20° of angular aperture. But the microscope objective had an angle of 120° of angular aperture and even more than that in some cases. Nobody suggested that Lommel's results applied to such apertures. Now, the important point was that the resolving power of a microscope was limited by the resolving power of the worst part of it; and consequently it would not do to say that we would only think of the diffraction which was produced by the back divergence angle.

Reversibility of the Microscope.

If then we had bad diffraction in front we could not get rid of that—the bad diffraction which happened in front of the lens. We should always be saddled with it, and the consequence was that unless the calculation was applied in

front of the lens as well as behind the lens, it did not tell us what we want to know. He might put the point in another way. The microscope, like every other optical instrument, was reversible. They could turn it the wrong way round and produce a small image from a big object. The power of the microscope to give you a well resolved small image under such conditions was exactly the same as its power to give a well resolved large image of a small object. Therefore the calculation of the diffraction in the case of that extremely wide angled pencil was a matter of capital importance. That was his answer to Mr. Selby's objection. With regard to Dr. Clay's objections, he thought that an interesting question was involved. Dr. Clay had suggested that the distances of bright and black dot images from one another in his (Mr. Gordon's) photograph would work out to the same as to the distances in the diagram which Dr. Clay had sketched upon the blackboard, and that the nearer they got to the source of light the greater would be the distance of the successive bright points. He entirely subscribed to Dr. Clay's theoretical proposition, but he thought that if Dr. Clay would do him the honour to look at the photograph which illustrated the paper he would see that that proposition did not in the least explain the photograph; and for the reason that the photographed images were all at so great relative distances from the source of light that the variations in their distances from one another due to the small increase in approximation of one against another to the source of light was something quite imperceptible.

Photographic Evidence.

In point of fact the distances between successive bright and dark spots were, as on Dr. Clay's rule they ought to be, measurably equal, but the observed focal plane was distant from the nearest dark spot not by a space only a little greater than this periodic distance—it was twice

as far away as on this theory it ought to be. He thought he was competent to say—he would not like to contradict Dr. Clay—but he had looked at it very critically, and had measured it with all the accuracy he could command; and it was capable of quite appreciable accuracy—he ventured to say that Dr. Clay would agree with him when he looked at the photograph and measured it, that the position of the observed focus had nothing to do with the law which Dr. Clay had referred to. The discrepancy between the displacement of the visual focus from its accepted position, as compared with the nearest black dot image, was a question of 100 per cent. —not of a fraction of 1 per cent., as it would be according to the theory which Dr. Clay had suggested. With regard to Mr. Smith's criticism of the diagram, that it was all wrong because the phases of light transmitted by a transparent dome were not the same at one moment through the whole of the area of the dome, as in the case of a wave front they were over the whole area of the wave front. That reminded him that he had missed a point, which was no doubt owing to his having broken away from his manuscript, and his answer to Mr. Smith was that it was perfectly true that the phases actually for a wave front were the same over the whole area at one and the same point of time, but as seen by a distant spectator they were apparently different, because of the different distances of the optical paths from various parts of the wave front to the spectator's eye. They had, therefore, speaking of *apparent* phases, a phenomenon exactly like that illustrated by the dome in the diagram —the arrangement was not the same, but the apparent phases were different in the wave front, just as the real phases were different in the case of the dome.

Conclusion.

There was, moreover, one other objection of Mr. Smith's which he might answer. He stated that his (Mr. Gordon's) cosine z, only affected a

portion, that is to say, the perspective dimension of a pane in the dome. It was the perspective dimensions he (Mr. Gordon) had been speaking about. The perspective dimensions of the wave front would also be affected by that cosine z. If they took a large section of a sphere, and if they did not stand at the centre, that cosine z must be taken into account. To get rid of it they must either take a small section of a sphere, or they must stand at the centre of the sphere, and in either of those cases the cosine z became $=1$. What he wished to emphasise was that the extracts read by Mr. Smith from Lommel's paper were not at all incompatible with that proposition. Lommel was tacit about it; as a matter of fact, the formula which he used was one which was limited to a very small spherical area, and he was therefore perfectly right in treating his cosine z as being equal to one, but that was on the condition that his results should be applied only to beams of a very small angular magnitude. If anyone cared to see it he had there a mercury globule giving a source of light which was in effect an artificial star. If they would examine that they would, by throwing it out of focus, be able to see the Fraunhofer ring, and one or perhaps two of the Fresnel rings very clearly.

NOTE, added February, 1912:—

Mr. Gordon desires to have it stated that since the reading of this paper he has succeeded in carrying through the integration illustrated by the diagram of fig. 1, and has by that means established on a theoretical basis the results referred to in this paper as demonstrated by experiment. He finds, however, that the system of rings illustrated in fig. 2, which occupy the aperture of a focused pencil of light, cannot be properly described as "Fresnel rings," or computed by the Fresnel-Airy mode of calculation. He desires this paper, therefore, to be read as if he had spoken of them as diffraction phenomena pure and simple, without any reference to the producing cause.

OPTICAL INSTRUMENTS FOR NAVAL PURPOSES.

By Naval Instructor T. Y. Baker, B.A., R.N.

Delivered on March 17th, 1910.

Among the many professions requiring the constant use of optical instruments of different sorts, that of the naval officer is one of the most important. The instruments he deals with are many and varied, some highly complex, and others wonderful examples of ingenious simplicity. In some, accuracy of design has to be pushed to its farthest limits; in others, very crude instruments will serve the purpose for which they are desired. But although great accuracy is not required in all cases, yet it must be remembered that the *raison d'être* of most of the instruments a naval officer uses is to ensure the safety of his own ship—or to encompass the destruction of the enemy's—both of which are matters of the highest importance; so that it will be looked upon as a pardonable error if the accuracy of design of some instruments is carried further than is absolutely necessary. It will, at all events, be an error on the right side.

All naval optical instruments can be grouped into two classes, the peaceful and the warlike; the sextant, the azimuth compass, and so on, which enable the officer to keep his ship off the rocks, on the one hand; the range-finder and the telescopic sights, on the other, which aid him to put a maximum number of shots into the enemy in a minimum of time.

Now, I am afraid that time will not permit me this evening to refer to the instruments of

the first class: besides, they have all been in use for so long, their mode of construction is so well known, and they answer their purpose so admirably, that we can almost say that the last word in their construction has been reached. Or perhaps I should say the last word but one, for, having had a certain amount to do with sextants, both in their use, and in the consideration of their correct design, I think certain small alterations might be made, that would render the instruments still more accurate than they are at present. That, however, is a point which I do not propose to go into to-night.

In the other class we have all the optical instruments connected with gunnery, and it is with them that I propose to deal, but I am not going to give you accurate and detailed descriptions of such instruments as are at present in use—some are kept secret and confidential by the Admiralty—others can all be gleaned from the various specifications in the Patent Office. Rather am I going to attempt to put before you the points, chiefly theoretical, which must be considered when starting the design of instruments to be used for the various purposes of fire control, to investigate the kind of accuracy that will be necessary in them, and to suggest the lines upon which, to my mind, the design should be carried out in order to achieve the best results. Of course, there are many ways of accomplishing the same end, and I think we may consider it axiomatic that we are most likely to get the best when there is collaboration between the user of the instrument on the one hand, who knows what he wants to do, and the instrument maker on the other, who knows many practical ways of doing it. Any attempt to design an optical instrument by the naval officer alone on the one hand, and the instrument maker alone on the other, is more likely than not to result in failure. I remember that some years ago I was shipmate with a naval officer who spent a very great deal of time, and

not a little money, in trying to perfect a range-finder, in which the method of adjustment was the focussing of the eyepiece of a telescope. He knew just enough about the theory of optics to be aware that $1/u+1/v=1/f$, and he spent, as I say, a lot of time in endeavouring to perfect a telescope that would turn the symbols of that equation into practical results. I am bound to confess that he was in the end partially successful, and that his range-finder did work reasonably well, up to a distance of about 100 feet.

On the other hand, I have no doubt that many instruments have been submitted to the Admiralty for trial which have been beautiful in themselves, but which, through the designer's never having had to use such instruments himself, have contained some foolish point of construction that has made the instrument absolutely useless. I remember seeing an artificial horizon that could be used at sea in misty weather, which used to swing through nearly four degrees when the ship rolled—so that one could not trust the latitude obtained by it much nearer than 150 miles. If the man who made it had known something about the period of a battleship's roll, he might perhaps have eliminated a good deal of the error, but as it was it gave a very good illustration of the folly of having instruments designed by people who have never had occasion to use them or to see them used.

In order to give you a clear idea of what is required of the various instruments when firing is going on, I propose to begin with, to explain shortly, what has to be done in order that officers may get an effective control of the firing of their guns.

In the first place, in order to hit the target, the officer in charge must determine its range, so that the gun may be elevated to the angle proper for the particular kind of gun, kind of shot, and kind of charge. Then, this range being known, the telescopic sights have to be set at

the suitable angle, and the muzzle of the gun elevated until the sights bear on the target. The sights are then horizontal, and the axis of the gun is at the right angle of elevation, and is ready to be fired. As firing continues, the range continually varies, and the sights have to be set up or down accordingly.

The optical problems to be solved are therefore the design of a range finder, and the design of a telescopic sight, and there are also two allied instruments; the rate of change indicator, and the spotting telescope. The former is used for the purpose of obtaining the rate of change of range, if possible, as a direct measurement instead of a difference measurement, and the latter for estimating, if the shot is a miss, how much it is short or over, for even if the range is obtained accurately, and the sights set accurately, and the gun laid accurately, there is always a varying factor to be considered in the temperature of the cordite, the amount of wear in the rifling of the gun, and other points, all of which have their effect on the muzzle velocity of the projectile, and consequently upon the range.

I propose, then, to consider in some detail these four instruments:—(1) Range finder, (2) Telescopic sight, (3) Rate of change indicator, (4) Spotting telescope.

Rangefinders.—To measure a distance accurately, some sort of base line is necessary, and the longer the base line in general the more accurately can the range be determined.

Now, every method of measuring with this base line must come in the end to the determination of a triangle in which the base and the two base angles are known.

In figure 1, suppose the angles θ, ϕ at A and B can be accurately measured, then, approximately, the range will be equal to

$$AB \sin \tfrac{1}{2} (\theta + \phi) \operatorname{cosec} (\theta - \phi),$$

and if the range is very great θ and ϕ would be very nearly equal to one another, so that they

would have to be very nicely measured in order to give the range accurately, since it depends on the sine of their difference. Further, AB sin $\frac{1}{2}(\theta + \phi)$ is approximately EF where EF is perpendicular to DC, D being the middle point of AB, so that the effective base line is EF, and not AB.

It is to be expected, therefore, that the results, with any given length of base line, will be best if that base line can be turned square with the line from the observer to the target, and that the longer base line we can get the better. The

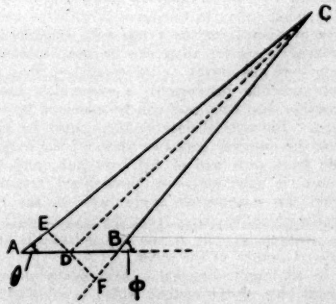

FIG. 1.

longest line possible in a ship is, I suppose, from a point somewhere in the bows to another in the stern, and in a battleship would be about 400 feet. But this would necessarily give an oblique base line, unless the ship were turned round to bring the target on the beam every time it was desired to take a range, and since the two points would hardly be within view of one another, every time a range was taken it would be necessary to measure the two azimuths and compute the range from their sum and dif-

ference. Taking the best case of the target being on the beam, and the range, say, 10,000 yards, the angle C would be about 45′, and a change of range of 100 yards would make a difference of about half a minute. To obtain this accuracy from such a range finder the azimuth observations would have to be made accurately to half a minute, and when we consider the possibility of the two observers not measuring to exactly the same point on the target—half a minute subtends 4 feet at 10,000 yards—we see what large chances there are for inaccuracy in a two-man range finder of this description, and, taken in conjunction with the necessity of calculating the range with considerable accuracy—greater than can be done mechanically—we are driven to the conclusion that the only possible instrument is one which has a movable base line, and can be operated by one man. The optical details, then, must be such that the observer gets two views of the target, one from each end of his base line, and the range is calculated—by mechanical means—from the convergent angle between the two beams which, starting from the target, go into the instrument, one at the one end of the base line, the other at the other.

In the next diagram I have given a very rough idea of the method which is adopted in the Barr and Stroud range finder, but the illustration is not correct as to details, I am merely putting the general optical principles into a convenient form.

If the object is the mast of a ship at an infinite distance, the two beams entering at A and B will be parallel, and as they are turned through right angles at A, B, and C, they will go out parallel, and the ship, as it is seen by the eye, will present the appearance as seen in the upper diagram (fig. 3).

If the two beams come from an object at finite distance, they will not emerge parallel, and we have the state of affairs as shown in

the lower diagram; and the angular distance which the break in the mast subtends at the eye will be the same as the angle subtended at the target by the base line of the range finder; omitting for the present the possibility of including a telescope by which this angular distance can be magnified up. Of course, in an actual instrument such a telescope is included; in the Barr and Stroud instrument there are two such telescopes, or, rather, two object

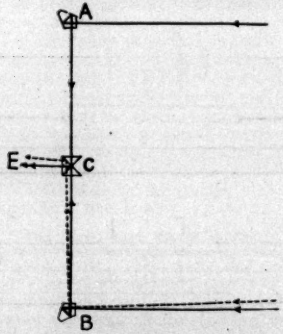

FIG. 2.

glasses, and a single eyepiece, the magnification being about 40 diameters.

To obtain the range, all that is necessary is to make one beam deviate by means of a prism until the two beams emerge parallel—make the two pieces of the mast, in fact, become collinear —and obtain some sort of measurement of the amount of this angular deviation. Then, since the range equals the length of the base line divided by the circular measure of the deviation, the range can be found. The most con-

venient method of effecting this deviation is to interpose a prism between one of the object glasses and the focal plane, and to move this prism along the axis of the telescope between the object glass and the focal plane. Then the amount of deviation is xD/f, where x is the distance of the prism in front of the focal plane, f the focal length of the object glass, and D the angle of minimum deviation of the prism.

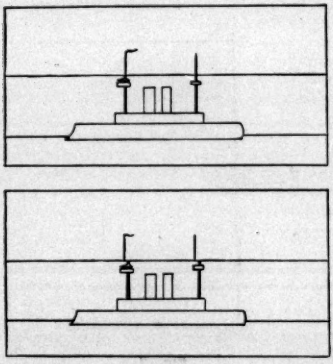

FIG. 3.

The distance x is then a measure of the angle of convergence, and is therefore inversely proportional to the range, so that if a reciprocal scale be attached to the prism, of a suitable size, the range can be read off on this against a fixed pointer.

That is, in principle, the way in which the Barr and Stroud instrument is worked, though, of course, there are many points in which elaboration of detail has been necessary.

Some such method as this is necessary in all instruments if this coincidence adjustment is to be used. Only one alternative method is at all possible, which is to use two equal rotating prisms in front of the objective—but in such case the mechanical arrangements for turning the convergence angle into the range would be distinctly more complicated, and the sources of error more numerous.

As regards the various points that have to be considered in such a range finder, the first, and one of the most important, is the length of the base line. The whole instrument has to be turned about in any direction, and also to be moved so as to compensate for the pitching or rolling of the ship. The control of the motion must be performed by the observer, who is also engaged in setting the prism for coincidence, so that it is clear that considerations of weight alone make it impossible to use a base line of more than ten feet if the base line is to be horizontal. I will consider the possibility of using a vertical base line later.

The 10-ft. Base Range Finder.

At 10,000 yards, a 10-foot base line subtends an angle of about 68″, so that a change of range of 100 yards will vary this angle by .68″; and the magnifying power of the telescope must be such that the break in the mast, which would naturally be .68″ wide, is magnified up so as to be visible.

There seems to be some considerable doubt as to what the eye is capable of performing with such an adjustment. I should say that it is the most accurate adjustment that can be made, using one eye only, and, as far as I have been able to verify by such experiments as I have made myself, my own accuracy is something of the order of 20″, so that though I believe Messrs. Barr and Stroud claim that much closer accuracy is possible, to make the observations with an accuracy of 100 yards at 10,000

a magnifying power of 30 to 40 diameters is needed in a 10-foot range finder—assuming that the instrument is quite free from errors.

The instrumental errors are fairly numerous. The zero—or, perhaps, I should say infinity—error is one which obviously can easily vary. It is necessary to put in more than the one prism, as otherwise it would not be possible to get down to the zero angle—and each prism is liable to displacement due to shock from firing the guns. Now, this shock of firing is a very bad one, in the respect that a whole series of shocks are administered to the instrument all in the same direction; and the various prisms and lenses are subject to a succession of jarring blows, all of the same type, whose effect is certainly likely to

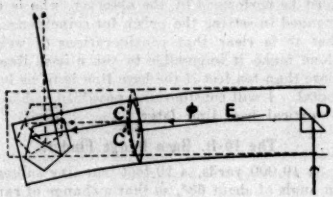

FIG. 4.

be cumulative; so that when testing a range finder to determine whether it is fit for the more or less rough usage it necessarily gets on board, some test of its performance under a repetition of shocks of the same nature ought to be included.

Another source of error is that due to unequal expansion under heat. If the tube containing the telescope is heated more on one side than the other, as, of course, happens in a strong sun, the tube will be bent in the manner shown in the exaggerated way in figure 4, and errors will arise in the readings. There will be no change in the deviation produced by the

pentagonal if it has been properly annealed, but there will be an error produced by the bodily displacement of the object glass. We have only to take what was the axial ray back to see that the angular displacement is equal to the displacement of the object glass divided by the focal length of the object glass.

In the first place, as regards the actual bending of the tube. If we take a brass tube (fig. 5), 10 feet long and 4 inches diameter, and warm up one side to be 1° hotter than the other, we have the following equations

$$r\theta = 10 \text{ feet.}$$

$$(r + 4\text{in.})\ \theta = 10\text{ft.} \times 1.0000187,$$

from which

$$\theta = .00056 \text{ radians,}$$

$$= 2' \text{ approximately.}$$

r being the radius of the circle into which the tube is bent, and θ the angle at the centre.

So that if one side of the tube is 1° hotter than the other there will be a bend in the tube equivalent to 2′ in its whole length.

The O.G. of the telescope is at C (fig. 4), and CED is the axis of the O.G. When the tube bends the O.G. goes from the dotted position to the position shewn by the full lines, and the amount of deviation produced in the ray is $CC^1/f = CE \times \phi/f = \frac{1}{2}\ CD\ \phi/f$.

Now $\phi = 2' \times CD/10$ ft., so that the deviation produced in the ray is $\frac{1}{2} \times 2' \times CD^2/120f = CD^2/2f$ seconds of arc where the distances are measured in inches.

This error will therefore be made small by diminishing CD, but in view of the motion that the prism requires the diminution of CD, cannot be carried out too far.

Suppose we take CD as 12 inches and f as 16 inches, the distortion produced by 1° difference of temperature will introduce an error of 144/32 seconds = 4.5″ The telescope on the other side would suffer in exactly the same way,

and the two errors would add up, so that the total error in the convergence angle would be 9″, which is equivalent to an error in the range of 1,200 yards at 10,000.

This shows how extremely important the temperature error is in such a delicate instrument as a range finder, and what very great precautions must be taken to minimise the effect of the difference of temperature which must necessarily arise when one side of the tube is exposed to greater heat than the other.

Among the methods which may be adopted to eliminate this temperature error are (i) the use of double tubes. This, I understand, is the principle adopted in Messrs. Barr and Stroud's range finder. The outer tube acts more or less as a shade to protect the inner one from unequal heating. This protection is made still greater by having the object glasses mounted on a framework which is clear of the inner tube, so

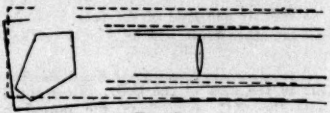

Fig. 5.

that the telescope is doubly protected. The precautions necessary must involve no greater difference of temperature in the actual tube in which the lens is mounted greater than about 1/30th of a degree Centigrade. (ii) A second method is so to arrange the design of the instrument that the two errors cut one another out, as they do, I believe, in the Cooke range finder. Such an arrangement is shown in the next diagram. I should think that it would hardly be likely to lead to good results in practice, as it would necessarily make the range finder very unwieldy.

Incidentally, this examination of the temperature error throws a little light on the won-

derfully accurate manner in which the telescope object glasses must be mounted up.

Referring to fig. 4, we see that the actual displacement of the object glass is $\frac{1}{2}CD^2 \sin 2^1/10$ ft. $= 3.6/10{,}000$ths of an inch. This displacement produces an error of 1,200 yards at 10,000, so that the displacement of the O.G., in order that the range error may be not greater than 100 yards at 10,000, must be less than 3/100,000 inch.

I think these few points will indicate to you how very carefully the design of a range finder with a 10-foot horizontal base line must be carried out if the instrument is to be good enough to show changes of range of 100 yards at 10,000.

FIG. 6.

Especially must the temperature errors be thoroughly investigated, and every possible effort made so to arrange the details of the instrument that errors will cut one another out, if possible, or alternatively be reduced to a minimum.

Vertical Base Line.

The second method of range finding that I propose to consider is one involving the use of a vertical base line. From the fire control platform down to the chart house there is a clear vertical space of 25 or 30 feet, and a range

finder might conceivably be made somewhat after the Cooke pattern, except that the base line is vertical. The pentagonals would be turned in the same direction and the temperature error could thereby be avoided, and if it were found possible to utilise a single object glass for the two beams deviating the light through, say, the upper half, for the adjustment, all trouble of the displacement of the object glass would be avoided also. In that respect the arrangement would be advantageous, but, on the other hand, there would be very serious disadvantages. Only one pentagonal would be required, but in view of the length of the base line the aperture of this pentagonal would have to be about six inches square, a size which becomes almost impossible when we consider the possibility of its becoming unequally heated and so distorted. Possibly something might be done in the way of utilising two mirrors, but the difficulty of mounting them so as not to have a variation of angle of more than half a second between them, would be very great. The error due to refraction would also present difficulties, and, on account of this error the instrument would require most careful setting every time it was used. But the greatest objection of all would lie in the weight. It could hardly be less than 100 lbs., and as the instrument would have to be free to swing as the ship rolled, it would be very difficult to control in bad weather. In a calm sea, I think the advantages of a long base line, and comparative freedom from temperature errors, would outweigh the refraction errors, but with the ship rolling, say, 10 degrees on either side, I am very much afraid that the instrument would take charge and become unmanageable.

Again, a third method of designing a range finder is on the stereoscopic principle. The shape and general outward appearance would be that of the Barr and Stroud, but the adjustment would be by the stereoscopic power of the

observer. With most people the power of stereoscopic vision is capable of being trained to a very nice degree of accuracy. Mr. C. W. S. Crawley, in a paper read before the Optical Convention in 1905, states that skilled observers might have an accuracy in this respect of 3″, that is to say, that if the angles subtended by the eyes at two objects close to one another differed by as little as 3″ a good observer could tell which of the two objects was in front of the other. Mr. Crawley's statement requires, I think, a certain amount of modification. For one thing, I cannot believe that one's stereoscopic error is the same at all distances. From a few experiments I have made, it seems to me that expressed in this angular manner, the error will be greater at long ranges than at short ones. My experiments showed that at a distance of about 10 feet, I could tell which of two objects was in front of another, when the distance in front was as little as $\frac{1}{8}$in. This gave the stereoscopic error as 5″. On the other hand, when the objects were moved to 40 feet, the least difference I could detect was five or six inches—a stereoscopic error 12″ or so. The experiments were not made with sufficient accuracy for me to trust those figures absolutely, but they do seem to me to indicate that the ordinary observer has a smaller stereoscopic error at small ranges than at longer ones. That, after all, is no more than we might expect, for stereoscopic power is something which is dependent upon our delicacy of perception of the muscular effort necessary to concentrate both eyes on a particular object, and, in general, people have been accustomed all their lives to exercise this sense of muscular control at short ranges, much more than at long ones. This delicacy of perception of muscular effort is not the only determining factor in dealing with the stereoscopic power of vision, but it certainly is one of the factors and, I think, an important one, so much so that I have no doubt that if a

man were to train himself to differentiate between objects at long range he could certainly reduce the stereoscopic error to something of the order of 2″—and he would be as much in advance of the ordinary man as the professional billiard player is of the amateur.

Education and Judgment of Depth.

As instances of what can be done in this way, it is quite certain that a man who has done a good deal of yachting has a much more accurate perception of differences of range in his own sport than a landsman has; or that a regular race-goer can pick out the leading horse as they come up the straight when an ordinary man would put them down as being all dead level.

I was told the other day that the captain of one of the steamers running between Portsmouth and Ryde claimed that when he stood on Ryde pier he could distinguish between the lamps on the one side of Southsea common and those on the other. Now, the distance is something like 8,000 yards, and the breadth of Southsea Common would be, I suppose, about 300—which works out as a stereoscopic error of about one-tenth of a second. I only had the story at second hand, and my informant was not sure of the exact circumstances, so I think that stereoscopic error can be taken with a grain of salt. One thing, however, I can see no reason to doubt—that the man had by more or less systematic training improved his stereoscopic vision considerably beyond the limits possessed by ordinary people.

Now, if it is possible to reduce the stereoscopic error at long ranges down to 2″, that should give a very considerable improvement upon the method of a coincidence adjustment, provided that the method of making the adjustment for the range is sufficiently good. This 2″ error would apply to two exactly similar objects lighted in exactly the same way, and it

is probable that it would be much higher if one of the objects was at a long range and the other were some artificial object in focus in the field of view. Doubtless means could be obtained by which this artificial object was brought to a state of perfection, and I shall not be at all surprised if a stereoscopic range finder in the hands of a highly trained observer does not ultimately beat every other type.

Telescopic Sights.—We come now to the instruments that have to be used to lay the gun accurately upon the target. An ordinary telescope, with cross wires in the eyepiece to mark the direction of the telescope's axis, which can be affixed to the gun at any required angle, will satisfy the conditions.

With the mechanical details of the mounting of the telescope, and the methods of setting up the sight to required angle of elevation, I think it will be unnecessary to deal. We will confine ourselves to the consideration of the telescope itself.

In the first place, when the gun is being fired, the most likely kind of miss is one short or over, due to incorrect elevation. A miss right or left is due to inaccurate allowance for the speed of one's own ship, and the enemy's, and is not usually so serious. Now, at 10,000 yards' range, a projectile travelling with an initial velocity of 3,000 feet per second, will require an elevation of 5° or 6°—and the variation in elevation for 100 yards change of range would be about 4′. For lower muzzle velocities the angle of elevation would be correspondingly higher, and the variation for the change of range also greater. The projectile would descend at an angle of 8° or 9°, so that a shot just clearing the top of a 30-foot target would strike the sea about 70 yards further on. Now, 70 yards in range means 3′ in elevation, so that to hit a target 30 feet high at 10,000 yards, the gun must be laid within 1½′ of the centre of the target. Making allowance for

certain small errors, we may take it, I think, that a man ought to be able to lay the gun within about 1′ of the point he is aiming at—or, if we are to consider the possibility of a man being trained to put his shots in special places on a 30ft. target, this accuracy of laying must be brought down to about ½′.

To the man who is laying the gun, the target appears to be continually moving about in the field of view of the telescope, owing to the rolling and pitching of his own ship, and if the controlling mechanism of the gun is not sufficiently delicate to enable him to follow absolutely the motion of the target with his cross wires, then he will have to fire whenever they come on—more or less fortuitously. This being the case, a fairly high magnification will be necessary—considerably higher than would be the case with a gun on a stationary platform. In fact, something like a magnification of 20

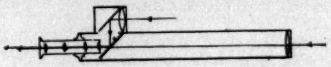

FIG. 7.

to 25 diameters will be desirable. Possibly a smaller magnification would do if the gun layer were very carefully trained.

For this purpose a 1½ or 1¾in. O.G. ought to be good enough, with a focal length of about 15 inches. With this magnification the angular field will be about 1⅓°, and, provided one's own ship is not rolling very much, the gun layer can keep his sights laid on the target without much difficulty. In a heavy sea, however, the ship's roll may easily be as much as 10° on either side of the steady position, and in such case it is exceedingly difficult to keep the target in the field of view at all, let alone keeping the cross wires on it. The period of roll of a battleship is about eight seconds from side to side, so that in this special case there would be an

average angular velocity of 2½° per second, or a maximum angular velocity of 3.9° per second. Consequently, with a magnification of 20 diameters, the apparent angular velocity of the target to the eye would be 78° per second when the elevating gear of the gun is stationary. This, of course, will be much reduced by careful control of the elevating gear, but, in any case, there is bound to be some sort of relative motion

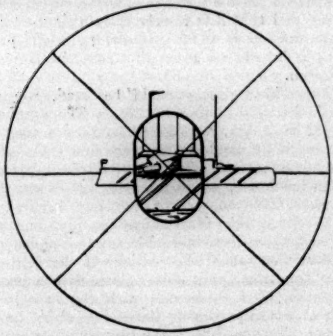

FIG. 8.

of the target and the cross wires, which it is desirable to reduce. Further, if you remember that if the man behind the gun ceases to control the elevating gear, the target will move off in the field of view at the rate of possibly 60° or 70° per second, and will consequently take something like a quarter of a second to disappear from view altogether; and that, when it once has gone, it is like looking for a needle in a bottle of hay to find it again, all this, I say, points inevitably to some sort of modification necessary in a telescopic sight for use in rough weather.

What we really want, in fact, is a telescope that will have a field of view of something like 10°, so that, with a partial control of the elevation, the target may be always kept in view. Of course, to do this with a magnification of 20 diameters, does not look very feasible, but there are two ways in which it might perhaps be possible to effect something of the sort. For one, remembering that the actively useful part of the field is simply the point of intersection of cross wires, and that it is merely there that the high magnification is wanted, something might perhaps be done in a manner illustrated in the next diagram.

Two object glasses would be used, one of 16in. focus, the other of 3in. The eyepiece would be of ¾in. focal length, giving a magnification of 21 diameters with the first O.G., and 4 with the second. Two plane mirrors, placed as in the figure, would take the light from the shorter O.G. into the eyepiece, and a hole cut in the lower mirror would allow light to pass through from the lower objective. The actual field of view would then consist of the field of view from the upper object glass, with a magnification of 4 diameters, with the exception that a certain portion in the middle would have a magnification of 21. The line of delimitation would be sharp at the sides, but blurred at the top and bottom. For use in fine weather, the low power telescope would be unnecessary, and could be bodily removed. In such a telescope the central portion of the field of view being of the high power, the full advantage of a high power telescopic sight would be obtained, and an effective field of 7° or 8°. The principal difficulty would be in the fact, that with the target moving about from the low power field into the high, it will disappear altogether for a short period, and will then appear with five times its previous velocity, and dash across the smaller field of view. Whether such a disadvantage would spoil the sight altogether

could, I think, only be determined by actual experiment. Then the second method by which the telescopic sight could have its field improved is by purposely introducing a bad distortion effect. This will not matter at the centre of the field, for the distortion there is zero; but if the distortion is made of the barrel type, and bad at that, it will effectively give a field of view in which the magnification is greater at the centre than at the outside, and it might be possible to enlarge the field of view, I should think, up to double the size consistent with the central magnification.

Both these schemes aim at keeping the high magnification at the centre of the field—that is, at the useful place. The alternative is to have a telescope whose field can be enlarged by the reduction of the magnification, which is the principle involved in the telescopic sights at present in use. Such telescopes are made of variable power by altering the positions of the lenses in the eyepiece, and with the ordinary four lens erecting eyepiece there are endless possibilities in the way of variations of the separations between the lenses.

The investigation of what is possible under such circumstances is much too long for me to attempt to go into; so I will leave the matter to such of you as may be interested in the case, merely pointing out the points that are desirable in a telescopic sight of variable power.

1. The sight should vary, if possible, between about 7 diameters and 21 diameters.
2. The adjustment for alteration of power should be a single one, *i.e.*, one motion should move all the moving lenses.
3. The field of view should remain in focus the whole time—and the cross wires also.
4. The eyepiece should be free from chromatic and spherical aberration for all powers, the other aberrations, since they do not appear at the centre of the field, are not so important.

5. For choice, the movement of the lenses should not require movement of the eye lens, so that to outward appearance the telescope is of the same length. A small amount of motion in the eye lens is not objectionable, but it should not be much.

Before leaving the subject of telescopic sights, I should like to say a word about the cross wires. Actual wires do not seem to me to be suitable for the purpose, and I think that a small circular spot etched on a glass diaphragm, and in focus—it would be best placed at the front focus of the eye lens—would be a better mark for aiming purposes—possibly a circular ring would be better still.

Rate of Change Indicators.—The instruments of the third class, the rate of change indicators, are, as I have said, not necessarily optical instruments. Their function is to determine the rate at which the range is altering, or, rather, to indicate when the range has increased or diminished by a given amount—ordinarily, 50 yards. Many methods can be employed to determine this rate of change, but they all of them fall into one or other of two classes:—(i) difference measurements either of the bearing of the target, or the masthead angle of the target, or the range itself even; or (ii) guesswork at the enemy's course and speed.

If the enemy's bearing from one's own ship be noted for three different instants, these three bearings, taken in conjunction with the known speed of one's own ship and one range, determined by the range finder, are sufficient to give the enemy's course and speed, provided that they are constant during the time between the first and last observation. Set down as a trigonometrical problem, it would doubtless give two troublesome equations, from which the course and speed could be determined, but treated as a geometrical problem, it is, as you may imagine, not a very difficult one; and, the

course and speed being known, the rate of change of range is also a problem capable of easy geometrical solution. But the accuracy necessarily depends on the accuracy of the measurements of the bearings, and if there are only small differences of bearing, the rate of change given by such means would be likely to contain grave inaccuracies. Of course, there is always the range finder to check the results by, but what is wanted, I presume, is something that will be good enough to check the range finder, rather than *vice versâ*.

To take a concrete case of the inaccuracies that can occur, suppose the enemy were steam-

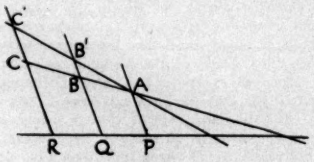

FIG. 9.

ing along A B C (fig. 9) at such speed as always to keep on the same bearing from one's own ship. P A, Q B, R C, are then parallel. If the three bearing observations are made at one minute intervals, and the course and speed of both ships are not varied, P Q will be equal to Q R and A B to B C—and if any other line to A B′ C′ be drawn, A B′ will be equal to B′ C′, so that the bearing observations will not discriminate between the line A B C, as the enemy's track, and and the line A B′ C′. Consequently, the rate of change cannot be deduced—it may be anything —and the only thing to do is to determine it as difference measurements by the range finder.

Any case in which the three bearings are not very different will consequently be liable to inaccuracy in the estimation of the rate of

change of range; but when the bearings are rapidly changing, then we shall get good results from this method. Unfortunately, the most likely state of affairs is, in a naval engagement, one in which two fleets steam parallel to one another, and at nearly equal speeds, so that bearings only change slowly, and the rate of change is liable to inaccuracy.

Another method of estimating the rate of change is from guessing the enemy's course and speed. At a distance of five miles it is not possible to form a very accurate guess at either, unless one has had considerable practice, but, in spite of that the method has surprisingly accurate results. Assuming that one can guess the enemy's course to within 10°—*i.e.*, the actual course is within 10° on either side of what is assumed—and that the actual speed is within four knots of the assumed speed—then the probable error in the rate of change is about 10 per cent. in the worst case. That is to say, the calculated rate of change would work out at 50 yards in 20 seconds, say, whereas it should really be 50 yards in 18 seconds—not a very great error. In such a case, after one accurate shot, there is a reasonable expectation of eight hits, and then a miss.

If we look at the question of estimation of the rate of change of range, from the point of view of designing an optical instrument for the purpose, we are forced, I think, to the conclusion that it must be an instrument involving a difference measurement. Bearings have the disadvantage that they are liable to serious error in the most important case—as also they can only be taken by the azimuth compass if the exact values are required—and that if they are not so taken, there will be errors due to the yawing of one's own ship; it is doubtful whether their differences could be trusted to half a degree, probably it is more like a degree; and the accuracy of the rate of change will be proportionately spoilt.

The Masthead Angle.

A second method is from difference measurements of the enemy's masthead angle. At 10,000 yards a mast 150 feet high will subtend an angle of 16′ 40″, and a change of range of 50 yards will produce a change of angle of 5″. Taking the base line to be sufficiently steady—that is to say, assuming that the rolling of the enemy is not too severe—we want a telescope that will measure the masthead angle, and which is so sensitive that changes of 5″ can be detected. The rate of change will then be a difference measurement, but it will be one which could, I think, be made with greater accuracy from the masthead angle than from the bearing—probably, also, with greater accuracy than the 10 per cent. error occurring in the method mentioned previously. The method of using the masthead angle is liable to failure in bad weather on account of the rolling of the enemy's ship, and consequent apparent variations in the masthead angle. Further, although the top of the mast is reasonably clear, the water line is not so, and it would probably be found necessary to measure to some other point, that is, to use a base line less than 150 feet—probably 100 to 120 feet. The length of the base line is, of course, unknown, so that an initial range is necessary to determine it. An instrument designed for such a purpose should be made so as to determine the height from a measured masthead angle, and given range, and subsequently to give ranges from measurements of the masthead angle. The instrument then becomes a range finder in itself, with a base line of 100 feet or so, which will certainly give more accurate difference measurements than a range-finder with a short base line. It has a very decided advantage, too, that as the instrument does not contain the base line in itself, it will be free from errors of the type of the temperature errors we have already considered.

Spotting Telescopes.—The last, and, I think,

the most important of all, is a telescope designed to estimate the distance that a shot is short or over. As I have shown, the gun-range is not necessarily the same as the true range, owing to a variety of causes. It is therefore highly important that the fire control officer should be able to measure the distance his trial shot is short or over, in order that the second may be a hit.

The splash made by the shot rises very high into the air, and the vapour and spray persist as a visible column for some considerable time, quite long enough to make a good observation of it.

The most reasonable mode of estimating the distance between the shot and the target is, I think, some form of a stereoscope. There is no clear edge to the column that would enable a coincidence adjustment to be made similar to that in the Barr and Stroud range finder, and methods which involve a measurement of the difference of the depression angle, as seen from the fire-control platform, down to the base of the target, and the foot of the splash, are bad, because both the marks are bad, especially in a choppy sea.

With a stereoscope having a 4-foot base line and a magnification of 20 diameters, the angle subtended at 10,000 yards is 27″, and a change of range of 50 yards is a variation in the angle of .13″. A magnification of 20 would make that 2.7″, which is the stereoscopic error allowable if the distance is to be measured accurately to 50 yards. This accuracy should, I think, be attainable with a practised observer, especially when we remember that the measurement will be made by adjusting some sort of pointer, firstly on the target and then on the splash. The convenient arrangement would, I think, be to have the adjustment of this pointer capable of two movements, each acting independently of the other. With the one movement the pointer could be kept continuously on the target, then

when the shot fell, it could be adjusted to the splash by the second movement, which would give the distance short or over, as a direct reading, if it were originally set to zero.

It should be noted that in such an instrument the splash and target would both be visible in the field of view at the same time, and that, irrespective of there being a pointer for exact measurements, the distance between the splash and the target could be estimated by eye. If the base line is taken to be approximately the eye distance multiplied by the magnification, the whole field of view will have exactly the same appearance of solidity as if the observer were very much closer to the target—in fact, at the distance equal to the range divided by the magnification.

If the magnification is 20 diameters, and the range 10,000 yards, the appearance of the target will be exactly the same as if viewed by the naked eye at 500 yards distant, and the distance that the shot appears short or over, as judged by eye, will be the correct distance up to about half-way between the apparent position and the target. It seems to me probable that if an officer were trained for a period of two or three months in the use of such a stereoscope he could estimate the distance of the splash from the target correct to about 30 yards up to 200 yards from the target, and to about 70 yards at 600 yards distant; and this without a pointer at all. An untrained man would, I think, estimate about as accurately near the target, but not so closely at bigger distances.

These, then, are the kind of instruments that are necessary to make naval gunnery effective. You yourselves will appreciate the very great delicacy of adjustment and accuracy of manufacture that is necessary if they are to be a real help to the naval officer, and not a hindrance, for you will doubtless see that if the instruments are inaccurate they will have not merely negative virtues but positive vices, in that they

throw the observer further out than if he trusted to his unaided eye. Especially is very great accuracy needed in the construction of the range finder if it is to give the true range correctly, though, as we have seen, this is not of so great importance as the gun range. It is an open question, in fact, whether in range finders the limit of accuracy has not been reached, and that, with present day instruments, whether the accuracy of the range finder is not considerably finer than the accuracy of the gun. For my own part, I should feel disposed to try to improve the spotting telescope rather than the range finder, especially with a view to obtaining one which would give a good stereoscopic view of the target and the splash, and in the use of which an officer could be carefully trained, and by which, as I have said, he could form very accurate estimates of the distance, short or over, by eye alone. The design of such an instrument would, I feel convinced, be of very great advantage.

After the spotting telescope, the telescopic sight seems to need further improvements. Such improvements must be in the direction of increasing the field of view, if possible, without affecting the magnification, and although such a problem looks at first sight to be impossible of solution, yet I think we may hope ultimately to attain the desired end.

Prof. Henderson said he was very flattered in being asked to open the discussion. The reason for this, he took it, was because he was a colleague of Mr. Baker's, and they had often discussed the points together; also on account of his former connection with Messrs. Barr and Stroud. He felt very rusty in optical work, as it was about ten years since he was engaged by Messrs. Barr and Stroud. They were at that time engaged in tests of the effects of temperature on range-finders, and all sorts of devices were employed for eliminating temperature influences. Many designs were drawn up,

some of which, on paper, seemed to be perfectly independent of temperature, like the one described by Mr. Baker with the two beams passing through the instrument together. A Naval range-finder must necessarily be an engineering design, and like all other engineering designs must be a compromise. One could start with an ideal physical design, but when it was found that it became cumbersome, or very expensive, or that errors were introduced by the engineering difficulties of construction, one had finally to make up one's mind whether to strive for perfection at any price, or go in for a practical instrument which had imperfections in its physical design, and then by careful engineering design and construction to reduce errors to within the limits of the observational ones. He thought Messrs. Barr and Stroud had produced a practical instrument which was found useful in service.

The resolving power of stereoscopic vision was referred to by Mr. Baker as 2.7 seconds. Helmholtz's work on the subject was now rather ancient, but he gave the accuracy as $1\frac{1}{2}$ minutes. Czapski gave the limit as about 53 seconds. Working on the coincidence method of adjustment, Zeiss gave the resolving power as 30 seconds. When he (the speaker) was at Barr and Stroud's they carried out a number of experiments and found that the accuracy of the ordinary coincidence methods with different observers was about 4 seconds. One observer was always accurate to within 2 seconds or 3 seconds. The allowance for uncertainty of coincidence in the Naval range-finder was half-a-second with a magnification of 30, which gave 15 seconds as the accuracy of the eye for coincidence adjustment. In ordinary practice they had no difficulty in working to half that deviation. The stereoscopic range-finder was dependent upon the accuracy of the two functions—the sensitiveness of the retina and the sensitiveness of the controlling muscles of the eyes. The

latter, he believed, was the factor which determined its final accuracy, because the muscles controlling the eye were very much affected by the state of the nerves; and though the stereoscopic range-finder had been found useful, he understood that even Messrs. Zeiss had abandoned it because of the difficulty of getting good and consistent observers. It was an extremely useful instrument, but it was necessary to have trained and good observers. The success of the Barr and Stroud range-finder lay in its beautiful construction from an engineering point of view. Although the instrument was originally designed in two hours, it was fully five years before the complete instrument was produced as a commercial success. He wished to express the very great pleasure Mr. Baker's paper had given him.

Mr. Conrad Beck said he had listened with great interest to Mr. Baker's paper, and he was sure the Society were very much indebted to Mr. Baker for bringing forward the subject in such a lucid manner, and for the information he had given them on the uses of naval optical instruments. The point that interested him more than anything else was that concerning the stereoscopic range-finder. It had always been a matter of surprise to him that it should be suggested the estimation of distance by means of stereoscopic vision was due to muscular action. He saw no reason why stereoscopic vision should not be considered as entirely due to the optical effect of the two images in the two eyes not being identically placed, because, in the case of comparing the relative distance of two objects, the examination of the two images, one in each eye, gave a clue to the whole phenomenon. If we call two objects one behind the other A and B, then one image of A may be made by rolling the eye to come to the exact yellow spot of the right eye, and the second image of A may be made to come to the exact yellow spot of the left eye, where will the two images

of the object B exist in the two eyes? If the object B is further away from the observer than A then the image of B in the right eye will be on one side of the yellow spot of the right eye, and on the other side of the yellow spot of the left eye, in both cases nearer the nose. If B is nearer than A then the image of B is on the opposite side of the yellow spot in both cases, further from the nose, and thus we judge of the relative distance of objects. The sole element necessary in judging the distance of objects is the power of discerning the relative position of the images of A and B in each eye.

A Novel Observation.

In the course of making experiments without range-finders he (Mr. Beck) and his brother had discovered an interesting phenomenon with stereoscopic range-finders which was not connected with the instrument, but was a feature of binocular vision. If they would allow him he would demonstrate this. (Describing on the blackboard.) If they took two white objects and placed them against a black background and then observed them at a suitable distance, they would find, with considerable accuracy, by stereoscopic vision, the position at which the observer must stand so that the two objects were equally distant from the eye. It would then be observed that if they confined their vision to one only of these objects, staring hard at it, the other one would not appear to be in the same plane, and then if they stared hard at the other the first one would not appear to be in the same plane. The two objects would, apparently, go backwards and forwards as the observer shifted his eyes. Whichever one they were focussing their eye on would appear in front of, or behind, the other, and whether it was in front or behind would depend on whether it was a black object on a white ground, or a white object on a black ground.

The exact explanation of this was rather diffi-

cult to determine, but the fact that the effect was reversed when the object was white on a black ground, or *vice versâ*, suggested that it might be a question as to which was the sharpest on the retina at the time, because, no doubt, the irradiation spreading from a white object on a black ground would be opposite to that of a black object on a white ground; but at any rate the effect was very marked, and it was quite easy to see it on any ordinary object out of doors. Take a pair of chimney pots at some distance away, it was difficult to tell exactly which was the nearest, for by staring first at one and then at the other, they appeared to jump backwards and forwards to a small but quite considerable extent. That was a physiological phenomenon which entered into the problem of the use of a stereoscopic range-finder, which, so far as he knew, had not been observed before, and had some considerable influence on the accuracy of the instrument.

As far as the resolving power of the stereoscope was concerned, he must say that he thought that the text-book limits which were given, as, for instance, those mentioned by Helmholtz, were far higher limits than have been found in the practical use of the stereoscopic range-finder. He thought two or three seconds was getting much nearer the mark than 20″ or 30″, when the resolution depended on either stereoscopic vision or the coincidence of the two images in a monocular range-finder, although 20″ might be the limit of resolving power for double stars. Stereoscopic telescopes, he thought, would probably have a great influence on range-finding questions, if they could be made sufficiently convenient for use on a rolling ship. The suggested gun-sight of Mr. Baker interested him very much, and it struck him that as the idea seemed to be not impossible to carry out, it would be interesting to know whether the Naval authorities had yet made any experiment in that direction.

Lieut. C. W. Swithinbank, R.N., said the remarks of the lecturer, especially those about the stereoscopic range-finder, were most interesting to him. The difficulty with Barr and Stroud's range-finder had been very great in many ways, especially in bad weather conditions. It had proved to be unwieldy with heavy seas, but he thought the spotting telescope on the stereoscopic principle was still more important. The range-finder was used more before firing commenced and the spotting telescope was used during the whole of the action; and if the telescope could be brought to such perfection that, with trained observers, the fall of shot could be as accurately gauged as Mr. Baker suggested, it would be a very great advantage. The question of stereoscopic training was one of very great importance. The limits of accuracy apparently were very uncertain, and a great deal of difference of opinion existed as as to the actual limit of accuracy, but he thought every one would agree that the accuracy could be very much increased with practice. Regarding gun sight telescopes the objection that Mr. Baker mentioned, *i.e.*, that the object would go out of the field in passing from the low power into the high power was very serious, especially in rough weather. The apparent speed of the target across the field would be suddenly increased, and it would be very difficult to control the gun to counteract this. The other method, producing very large distortion, was very interesting, indeed. There was no reason for doing away with all the aberrations on the outside of the field, as it would not matter how much distortion occurred at the edge of the field. It would be very interesting if they could get a telescope free from errors at the centre of the field with large magnification, and which would still give a comparatively large field.

Mr. Reiner said the matter mentioned by Mr. Beck was dealt with in the 'Lancet' of February, 1908, on the question of distant

stereoscopic testing with two rods, one 130 mm. behind the other, and 40 mm. laterally apart, and at 20 meters from the observer. The result was that the eye, if corrected to normal, could see distinctly that one rod was in front of the other; but at any other distance the objects could not be seen as one in front of the other. With regard to the sighting telescope, he asked if it would not be possible to use both eyes, getting with one eye a very wide field, and with the other a small field.

MR. BLAKESLEY said he thought the paper was very interesting. There were one or two points which occurred to him, and he would like some light thrown upon them. For instance, with regard to the very interesting remark made by the reader of the second paper. The red screens had been used to view distant objects, and it was found that they cleared up what was previously obscured by haze. The real explanation of that was that the red glass, when used as a screen was superior to a green glass, because they were more likely to get monochromatic light through it than through a green glass. According to the theory of colour vision, one set of nerves were used as far as we generally see in the red and began to be affected by the "A" rays, and went upwards to a point which different experimenters had different views about. When the rays of light got to the highest point affecting one set of nerves, another set of nerves came into play. When dealing with green light it affected two, if not three, sets of nerves, so that they could not have—at least, he had never seen it—a pure green light produced artificially. Even if the spectrum were used, they got different sets of nerves affected by it. He knew the spectrum pretty well, and there was a curious experiment which he had often done with it. If he passed his eyes along the spectrum and took a spectroscope of high red light for a minute or so, then turned to the higher spectrum and tried to trace the lines

where he thought the "D" lines ought to be, he found the actual focus invariably stopped short of them. That very interesting point had been illustrated lately in the unfortunate case of Mr. Trattles, and he thought it was one which the authorities ought to study more than they seemed to have done. This poor man is told that he is not blind, but the authorities seem to know very little about the case. If they only understood a little more as to what actually happened on the retina, they would see that it was possible to get a red light which was perfectly monochromatic, and then, of course, objects viewed through this, attached to the telescope, would be cleared up, because there would be no chromatic aberration in that case. Some time ago he was engaged with another point which the reader of the first paper spoke about, *i.e.*, the displacement which was liable to take place when the telescope which was used for accurate work was jolted, as in the case of firearms.

As the matter was a rather important one to him, he went into the investigation how to get over this difficulty. The method which occurred to him was the following: If they had a telescope object glass made, of course, of two glasses, for the purpose of achromatising it, and then, if they could by any artifice lengthen the second of the two glasses, not a short distance, but right away until they got to the focus, they could then increase their fiduciary lines upon the surface, and magnify them in any way they chose by the eye-piece. He was fortunate in securing the services of Mr. Beck in producing this instrument, and it fulfilled his expectations very well indeed. The whole thing was apparently a cylinder of glass cemented tightly and otherwise secured in a brass tube, which in its turn was secured on the rifle. No jolting could possibly take place there, otherwise the whole thing would have given way entirely. The great drawback in telescope sights

was their liability to be displaced. They could get two lenses, but they must not be screwed too tightly together, otherwise they strain the hair lines or spider lines which latter were not very substantially attached to the frame, being screwed through the sides of the telescope. This seemed to him to open up the chance of movement very largely indeed. When he used that little instrument which he had described on a rifle, although he was not a rifle shot before, he made some scoring with it which he would not have been able to do without it. The eyepiece of that instrument was a peculiar one, it was a Ramsden, in fact. They must get some plan of focussing for different eyes, and if they focussed an eye-piece they would not be able to lend the instrument to a friend. In the Ramsden eyepiece he cemented the first of the two lenses straight on to the long piece of glass of the telescope, and that secured it from injury. It never gave way in all the instruments he made with it, and for focussing purposes, when used by different eyes, the Ramsden glass was used. He believed that was the instrument, although he had not used it for some years, he believed he could put it on a rifle and get a bull's eye at 500 yards without taking a sighting shot with the rifle at all.

Mr. T. Smith said he should just like to ask Mr. Baker whether there might not be some difficulties if, instead of using cross lines, or cross wires, in a gun sight, a spot or a circle was used as Mr. Baker suggested. He thought that it would not be nearly so convenient in use at night. He would also like to suggest that the horizontal and vertical lines were a much better arrangement when one instrument was used for setting the elevation and the other for direction of fire.

Mr. Chalmers said, before calling on Mr. Baker to reply, he would like to make one or two remarks. He thought it was a matter of very great regret that a number of the members

of the Society who were interested in visual acuity questions were not present, because he believed that the very great accuracy obtained in stereoscopic vision had a very disturbing effect upon many of the theories which they had accepted for many years. They found, and many experiments had verified the results, that it was possible to distinguish angular values which were certainly less than 6″, and some observers had been able to get with certainty down to as low as 2″. He thought that was a very disturbing influence on the theories of physiological optics, and they were living upon the work of the great thinkers of the past, and were accepting their statements without verification. With regard to the portion of Mr. Baker's address touching upon spotting telescopes, he (Dr. Chalmers) thought the spotting telescope should be arranged to give an absolutely perfect picture, real in size and real in every way. He thought most of the stereoscopic range-finders had failed because the pictures were not real, but gave exaggerated stereoscopic effects at different distances, and that was because the designers of stereoscopic range-finders had not realised that the converging angles of any definite object were increased very much beyond what they should be. In fact, when one used the magnification of the telescope and the increased separation of prisms, one was magnifying the angle subtended by the object much more than one should. There ought to be prismatic adjustment for the purpose of destroying a great part of this convergence. It should be arranged that the actual convergence for the object on which they were really focussing should be the same as if a near object were seen directly. Then their picture would be a real picture, and they would be able to judge distance just in the same way when they reduced the distance by the magnification of the telescope. In that way he thought they could give the training for the spotting telescope without the necessity for em-

ploying the actual instruments themselves. They could teach people to judge 500 yards. There were many people who were in the habit of judging at that distance with very great accuracy. Then, if they could make the picture, as it was seen in the stereoscopic spotting telescope, a complete reproduction of the object, it would be seen at 500 yards instead of 10,000 yards. The actual amount of training necessary was very much less than one would think. He was much interested in the results of the observations of Messrs. Lucking and Boatman. Their first set gave a record of 6″ in the one case for the stereoscopic limit. The next set gave a limit of about 4″. That 4″ had been obtained after a number of observations not made under the same conditions, so that there was a marked improvement with a little practice; and he believed the best stereoscopic accuracy could be obtained with a little practice. There was one other point he would like to call attention to. Mr. Beck, in attempting to repeat his observations, found it impossible to get the effects unless he looked at the object beyond the time he would ordinarily look at it. He (the speaker) had tried it, and had succeeded in getting results similar to Mr. Beck's, but in the ordinary way he did not think it occurred unless one deliberately stared at the objects, and forced one's self to see it. In conclusion, he would like to thank Mr. Baker for the very great trouble he had taken in bringing forward the matter, and to assure him that a paper of that sort would have a very great influence on optical instruments for naval purposes. Ideas like those brought forward were no doubt talked about among opticians, and ultimately were incorporated in the instruments used in the gunnery section of the Naval Service.

It seemed to be quite definite that if one used mono-chromatic light for ordinary objects there was a very pronounced increase in the stereoscopic effect; but ap-

parently, when it was intended to see definite white objects on black ground, it was found, he thought, at first, that the improvement was small; and Mr. Lucking had suggested this, that it was possibly a question of chromatic aberration of the eye. If that was the case, the elimination of that chromatic aberration should have been equally favourable upon objects purely black and purely white. That might have something to do with the matter, but he thought the improvement was due to a very great extent to the increased sharpness of the contrasts obtained when one looked through red glass at ordinary commonplace objects. Perhaps it might be desirable to try some of the stereoscopic range-finders used at sea, whether the insertion of a plate of red glass would be beneficial in a case where there was sufficient light to see the object being observed. In the case of the slides which had been shown them, for those experiments they had used tri-colour screens.

Mr. Baker, replying to the various questions brought up, said, the subject had been a matter of very great interest in him, and he was sure that anyone who took it up would also find it very interesting. First, Professor Henderson mentioned that several opticians gave the coincidence adjustment as low as 10″. It was possible that that amount might be reached by a specialist who had been trained for some little time in making experiments; but unless one had had that considerable training it was rather a finer accuracy than the ordinary man could attain. Just in the same way as with stereoscopic power, it was a matter of training. When he was at sea on one occasion several officers tried the range-finder on a well defined object—at something like 5,000 to 6,000 yards distance—and there was a very considerable divergence of opinion as to what the actual range was, and this divergence of opinion amounted to a matter of 600 yards. If they worked out that

error they would find it came out very much larger than the 10″ or 15″, of which Mr. Henderson spoke. With a 4′ 6″ range-finder they would find the error worked out at something like two minutes.

Then with regard to what Mr. Conrad Beck said with reference to stereoscopic vision not being a matter of muscular adjustment so much as the actual idea of what one got of the image formed on the back of the retina, it seemed to him that it was only a small point as to whether it was muscular control or not. In any case, it was something which was capable of improvement by training, and that stereoscopic error could be made very much finer indeed, if one had had a course of training. The experiments made by Messrs. Boatman and Lucking seemed to confirm that fact, that the stereoscopic error could, with a little bit of practice, be considerably minimised. They must have had a great deal of practice before they got together the results which had been shown on the slide. These seemed to point to the fact that stereoscopic error could be made as small as 2″ or 3″. He noticed the experiments were all made at the same distance. Eleven meters was not the range wanted for actual practical training for a naval officer. The kind of thing desirable seemed to be a system of stereoscopic vision of 500 yards, as Mr. Chalmers had suggested. As regarded Mr. Beck's experiment with the two pointers, was it not possible that that effect was produced by fatigue of the nerves of the retina, and that, as Messrs. Lucking and Boatman said, the best results could be obtained if one gave judgment immediately. If the observation were a long one, an accurate judgment was not so likely to be formed.

As regards the actual use of optical instruments, the range-finder was used initially before the firing began. After that the important instrument was the spotting telescope. The accuracy of the range-finder was not, after all, so

important. One might have an accurate range, but there might be various errors in the gun—the temperature of the cordite, the way in which the projectile or cordite was rammed home, all these would have a tendency to make the range faulty, so that it was not so much a question of the actual range-finder—the range would still have to be found by means of some sort of trial shot—and what was really required was an accurate spotting instrument. If he were an instrument maker and were going to devote his whole time to building up an instrument for naval purposes he should certainly set about making a spotting telescope, rather than an accurate range-finder. When he was at sea he found that with the range-finder they might get it as accurate as possible before firing began, but the first shot might easily be 500 or 600 yards out at 6,000 yards. With regard to his tentative suggestion for a method of getting a double field in a telescope sight, the question as to whether such an instrument would be practicable was not a matter that could be worked out on paper. It could only be tried by building a telescope on the lines he had indicated.

Mr. Blakesley's account of the telescope sight he made for a rifle was interesting, but he should doubt whether it was possible to do the same thing with a telescope to be attached to a gun. In the first place, with a rifle it would probably be found that such a high magnification would not be required, and if they were going to get a magnification of 20 or 25 diameters, they must necessarily have a very big object glass; and by the time they had lengthened out the object glass until it reached the focal plane they had got a pretty big thing, and the light lost would be considerable.

Regarding what Mr. Smith said about the use of the circle or spot of light for the purposes of shooting, he could see no reason why that spot or circle could not be etched upon the glass and the glass illuminated from the top.

This would have the effect of just illuminating the spot and still leave the rest of the field more or less visible. Of course, this would mean a telescope with a magnification of 20 diameters, which was rather a tall order for use at night time, and probably some absolutely different telescope sight would have to be used for night purposes.

There was one other point mentioned by Mr. Chalmers, with regard to the magnification of the convergence angle. In using the telescope what Mr. Chalmers stated, undoubtedly would be the case. One would certainly get a solid appearance in the field of view, but it would also require a very considerable strain on the muscles of the eye, because the convergence angle would be magnified up also to the same amount as the field of view; so that it would be necessary, he thought, to introduce, in addition to the two telescopes, some sort of adjustable prism inside one of the telescopes on which that convergence angle could be reduced to any required value afterwards. If a prism were put in one of the telescopes so as to move the field of view in one telescope relative to the other, it might be possible to move the convergent angle to what one wished in such circumstances. The effect upon the field of view would be to vary the size of the object looked at. On some occasions some position of the prism would correspond to the whole field of view in a definite scale; but with a suitable adjustment of such a prism he thought it would be possible to get the whole field of view still to appear solid and to appear the right size; the size they would actually have if the observer were at a position something like 500 yards from the target. If that were done he thought it would be possible to judge the distance by the eye.

ON SOME MEASUREMENTS OF STEREOSCOPIC POWER.

By D. P. Boatman and R. J. Lucking.

Read March 17th, 1910.

During the course of his interesting lecture this evening, Mr. Baker has raised the question of the accuracy, with which we are able by means of stereoscopic power, to judge of the distance of various objects.

It is in order that members may, if they wish, judge of this accuracy for themselves, that we have arranged to make a series of simple tests, with a view of obtaining, as far as possible, a general idea of the smallest separation which can reasonably be taken as the limit, beyond which stereoscopic power fails to give an indication of a difference in distance between any two objects observed.

During the last few weeks, we have been conducting a set of similar experiments in our laboratories at the Northampton Institute. We found, as is only natural, that the limit varied considerably with different observers.

In the first slide are tabulated the results of one of these experiments. They indicate that the observer's limit, in this particular case, was four seconds of angular value, corresponding to a separation of the two objects of four cms. over a range of eleven meters.

In the case of the observer whose results are shown in the second slide, the limit was reduced to differences in distance of two cms. over the same range, which is equivalent to two seconds of angular value.

It must not be supposed that these limits were

fixed from these sets of readings alone. They were only arrived at after three or four such sets had been taken in each case.

Mr. Crawley's experiments, which were mentioned by Mr. Baker, were of a somewhat different nature, consisting in setting two objects to coincidence, and not as in the case of our experiments, of judging two objects already set.

While engaged in these experiments, we noticed, on looking through variously coloured glasses at objects situated some distance away, that there was, apparently, a marked increase in the stereoscopic relief. This was especially noticeable in the case of red glass, a fact which was confirmed by several students and others to whom we submitted the test, although we asked no leading questions, which might have influenced their judgment one way or the other.

The objects viewed were the branches of some trees, and it was impossible to definitely say which branch was the nearer of any two taken, when viewing in the ordinary way. As soon, however, as the red glass was brought into use, our doubts disappeared, and we were quite confident which of the two was actually situated nearer to us.

On attempting to verify this apparent increase in the stereoscopic relief by actual measurement, we found, using the same apparatus and working under the same conditions as in the former tests, that the results, although they tended to show an increase in the acuity, were of such a variable character, that we were unable to rely on them to give a limit in the same way that we could rely on the former tests to do so.

To account for this failure to establish a proof of our assumption, we compared the conditions under which we conducted the tests, with those under which our first observation of the phenomena was made. We found that there existed two differences which might easily account for our failure. They were as follows:

In the first place, there is always present in the atmosphere a blue haze, which tends to tone down the sharpness of the edges of objects viewed at a distance. This haze, on account of our short range (eleven meters) was not present to any appreciable extent, the edges of our bright white objects being sharply defined against the black background used.

On the other hand, in the open air and over the comparatively long range, this mist was most noticeable, so that the red glass cutting, as it does, the blue rays out, would sharpen up the definition considerably. Moreover, this would explain to some extent why it is that, on viewing through other colour glasses, the effect becomes less noticeable as we go towards the blue end of the spectrum, until finally (using a blue glass) *no* increase in stereoscopic effect was observed to take place.

In the second place, on account of our using artificial lighting, the total illumination was very much less than in the other case. Consequently, in accordance with the well known fact that the visual acuity falls off very rapidly when using red glass, if the illumination be reduced, our binocular perception was not so keen as it might have been, had we been able to conduct the tests in daylight.

Having failed, then, to make a measurement of this apparent increase in the stereoscopic relief, and in view of the differences in the conditions of working, mentioned above, we decided that the probability is that the red glass, providing a practically monochromatic condition, such as it is not possible to obtain with other coloured glasses, sharpened up the edges of the image, thus providing better conditions for judging the effect.

If this supposition is correct, it will be seen that the use of red glass in the construction of outdoor measuring instruments which depend on stereoscopic vision (such as range finders, for instance) might be of great value, especially if

it is going to increase their accuracy to the extent indicated. That is, about 30.

While working with the red glass, Mr. Boatman was asked why, if red glass clears up the image to such an extent and also adds to the stereoscopic relief, red glasses were not prescribed in place of chlorophyll or blue glasses?

The reason is not far to seek, and, as he replied, not only is the red glass far from being as restful as the other colours, but it is obvious on looking through the red glass at various colours, that the absolutely monochromatic light makes it quite impossible to distinguish any difference in colour between a violet article and a red, the only difference being a difference in saturation apparently. Besides this, on taking the glasses off after wearing them for a week or two, everything would appear to be of a greenish hue owing to the extreme fatigue of the retina, which might not pass away for some time.

Then, again, there might be a slight effect on the wearer's general health, although this is doubtful. At all events, the appearance of the patient would not be improved.

During a discussion on the effect produced on the patient's general health, Mr. Boatman related to me an anecdote connected with a tyrannical Tzar of Russia, whose favourite practice seems to have been to confine his political prisoners in a cell illuminated by red light only, which treatment is said to have ultimately reduced them to a condition of insanity. This experiment, needless to add, we have not verified.

Apparatus.

The apparatus with which we made our experiments, and which we have roughly fitted up here, has the merit, at any rate, of extreme simplicity. It consists of two white objects, whose relative positions can be read off by means of two scales, drawn a fixed distance apart, on a sheet of white paper, and graduated in centimeters, the paper being fastened to an ordinary drawing board.

It is necessary, in order that the opinion of the observer shall be, as far as possible, unbiassed by general appearances and surrounding objects, to fulfil two conditions.

(1) There must be no sharp edges on the objects themselves, lest shadows should be cast on the surface of the objects, which would give indications of their positions, with reference to the source of light used.

(2) There must be a difference in the heights of the objects, otherwise perspective might influence the decision.

In addition to these two precautions, it is necessary to cover up the bases of the objects, as it is obvious that their positions on the surface of the board would form a clue.

Chalk Objects.

For various reasons we chose chalks as objects. Being circular in cross section, they fulfil our first condition, and only need to be cut down slightly to fulfil our second. Also they are convenient as being easily replaced if broken, thus enabling us to reproduce the same conditions of working at any time, and it would be difficult to obtain other bodies so uniformly white as chalks.

One thing the observer must do, and that is, form his opinion *at once*. Otherwise he unconsciously allows himself to be influenced by the surrounding objects, however much care may be taken to prevent such undesirable influences coming into play.

ON ABRASIVES AND POLISHING MATERIALS.

By Walter Rosenhain, B.A., D.Sc.

Delivered on April 21st, 1910.

The lecturer said that the subject of abrasives and polishing materials was one of the greatest importance to all those connected with the finishing of metals and glass, and he proposed to deal with it, in view of the Society being an optical one, from the standpoint of the polishing of glass. There was very much room for the scientific study of the subject, because the practice which was followed in the polishing industry was to a very large extent a matter of rule-of-thumb. Preparations were often purchased in the market without very much enquiry as to how they were made or what properties they possessed. These were tried and found moderately satisfactory, and seemed to be retained from the fear that if they were to make a change they might get something which was not even so satisfactory. At the same time there could be no doubt that careful and scientific study of the subject would be of the greatest possible value.

An analogy might be drawn from the experience of engineers with their cutting tools. For a very long time their cutting tools were simply made of tool steel, made in a rule-of-thumb fashion, handed down from their grandfathers, until they began to realise the importance of cutting tools. As a result of scientific investigation they had now got cutting tools which were perfectly amazing as compared with the cutting

tools of twenty years ago. They must remember that every particle of an abrasive was a cutting tool, and just as the engineer would not now use a cutting tool without knowing its nature, so in the same way it was desirable for efficiency and for securing the best possible results to study the nature of their particular cutting tools, that was to say, the particles of abrasives. His object in preparing that paper was to put together some of the well-known facts about abrasives, but, unfortunately, circumstances had been against him, and he was afraid that any new features must be left till a later date. He would begin by dealing with abrasives, considering their mode of action, and the essential property of the material to be abraded. That essential property was the mode of fracture or breakage of the material.

There were two large classes of materials in the world, and, of course, a good many intermediate qualities of matter; but practically speaking, there were two large classes, one of which he might call the vitreous, the amorphous —more or less solidified liquid, and the other the crystalline, and in many cases ductile. If they took a crystalline material, they would find that it broke with a very good line of crystalline fracture. If they took a piece of glass and broke it, they would find a very typical fracture of a vitreous body, which was called conchoidal, because it had a distinct resemblance to the surface markings of certain kinds of shells. How did this mode of fracture affect the action of an abrasive? The answer lay in the fact that any cutting of whatever kind was done by the production of local fractures.

If they took a chisel and applied it to a piece of material, such as a piece of wood, and struck a blow or applied pressure to the back of the chisel, they were exerting a pressure upon the wood at the point where it came in contact with the chisel. If the chisel was sharp the area of contact was extremely

minute, and the result was that on that minute area a very heavy pressure was exerted, a pressure which, if calculated out in tons per square inch, might run out into many hundreds; consequently the pressure exceeded the breaking point of the material, and then if the chisel was pushed and the pressure continued the piece of wood was cut through. That was the *modus operandi* of all cutting operations. It consisted essentially of applying, by means of a sharp tool, a very severe local pressure to a restricted area, with the effect of producing crushing and fracture at that point.

That described the nature of the action of a cutting tool on metal; it was also the action of an abrasive, but in the latter case it was not one tool, but every grain of the abrasive which acted as a cutting tool, and these grains produced the local fracture at the point of contact. They would see, therefore, that whether a tool or a piece of abrasive material produced a cut or merely an indentation depended upon its sharpness, and the actual area in contact between the tool and the work. That was why the size and shape of an abrasive affected the manner of its action; and to show that the action of grinding really produced nothing more or less than a series of local fractures he had a couple of lantern slides which he would show, in which the surface of ground glass was seen to be covered with conchoidal fractures.

Having realised the manner in which the abrasive was to cut, the question was how was it to be applied, and there were three ways of applying an abrasive. The first one was to throw the abrasive at the work, as, for instance, with the use of a sand-blast. The second was to fix the abrasive in a wheel or disc or revolving object of some kind, and hold the work against it. The third method was to have a revolving disc of metal and to hold the glass against it and feed in the abrasive as a paste. How did it act in those three ways?

If they threw the abrasive at the surface of the glass they were simply doing on a very small scale what was done when one hit a piece of glass with a hammer—the abrasive had the same effect as a slight blow. They would see that its efficiency depended not so much on the hardness of the abrasive as upon its weight or density. The energy of the blow was what told, and the smallness of the area in contact between the striking particle and the surface of the glass. Consequently, tiny steel balls or steel shot made a much better abrasive than the ordinary grinding materials for that purpose.

In the second method they had a wheel or disc composed of an agglomeration of grains—a familiar example of which was a grindstone. This wheel was generally rotated at a very high speed, and the work was held up against it. What happened was that at the first moment of touch one or more grains of the abrasive struck the surface of the glass and produced fracture cones, running along the surface of the glass, and resulting in the formation of a groove. That groove was immediately interfered with by the formation of other grooves, owing to the momentary impact of the particles of abrasive, and they got all the glass covered by the conchoidal fractures on the surface. They would see, from what he had said before, that the need for hardness in an abrasive was easily understood. He had shown them how the concentration of stress produced an intensity of pressure which exceeded the breaking point of the material which was being cut. It was essential that the breaking strength of the material of the tool itself should not be exceeded, otherwise the tool would not do the work. And if they tried to cut a piece of hardened steel with an ordinary knife the tool steel itself would fail, because the concentration of pressure would cause something to give way, and the weaker vessel would go to the wall. Hardness after all was only a form of strength and similarly in an abrasive, as an

abrasive was only an aggregation of a number of minute cutting tools, each of them required to be hard and strong enough to withstand the pressure which the cutting involved. Not only that, but if the tool was to be applied in the way of a fixed wheel it must be sharp and be possessed of very acute angles, because only by the possession of very acute angles could they exert sufficiently intense local pressure to cause fractures. It was possible to abrade with sandstone, but it did not do the work nearly so quickly.

The third method of abrading was by feeding an abrasive in the form of a paste between a revolving tool and the work, and this was almost universally used in grinding glass. He imagined the reason for this was because the tools could be accurately figured to the required curve, whereas if they were to try and produce curves by means of abrasive discs it would mean a separate abrasive disc for every curve, and those were only shaped with considerable difficulty and would have to be re-shaped for every separate purpose. He was not at all certain that for some purposes at any rate that method of grinding did not deserve a great deal more attention than it received at the hands of opticians. The objection to the process of feeding an abrasive as a thin paste was that the individual grains of the abrasive were not held up to their work in the same positive manner as in an abrasive disc. It was true that the process drove the grains into the surface of the metal tool, and that others were thrown about backwards and forwards between the work and the tool, but then another thing happened; the abrasive was partially squeezed out between the work and the tool and a great number of grains were driven out before they had done their work. Consequently the abrasive was damaged by debris and broken glass. The fine particles of glass accumulating in the paste carried some of the pressure that was being applied, and instead of having an intense local pressure on every point

of the abrasive, a great deal of that pressure was borne by the agglomeration of glass particles.

In the use of the abrasive as a sandblast by throwing it at the work, he had shown them that the density of the particles mattered a great deal, and, therefore, metallic materials like steel were satisfactory. They might then consider what were the desiderata for abrasives to be used in a sandblast—their density, sufficent hardness and their approximate spherical shape. They did not want sharpness for that, because if they had sharp particles it meant that they must have flat sides, so that the best shape was spherical, or approximately spherical. On the other hand, for abrasion by the fixed disc they wanted extreme sharpness, very great hardness and strength, while the density of the material was quite a minor matter when they were revolving the wheel quickly. In the case of an abrasive fed on between the tool and work the conditions were the same, except that the extreme sharpness in the angles did not matter so much, because they were relying on chance in driving the angles at the work instead of the sides of the grains.

Probably the oldest abrasive, and certainly the cheapest, was sand. Sand was used a very great deal, probably more than some of them imagined, in grinding glass, and he knew that glass could be ground, polished and finished with sand, followed by rouge, so that it was not by any means a thing to be neglected. At the same time, although it was very cheap and plentiful, it was not very efficient. The reason was that sand was almost pure silica, and the hardness of silica was far inferior to oxide of alumina or carborundum, or several other things that were well-known as abrasives nowadays. Sand was very rarely found to be pure silica, being nearly always mixed with felspar or other matter, and sand being water-worn the grains were always more or less rounded. They never

got a sharp sand in the sense of sand having sharp angles.

The next of the well-known abrasives was emery. It was only an impure form of oxide of aluminium, and was of the same species as ruby and emerald. That part of it which was oxide of aluminium was extremely hard and had very sharp angles, but natural emery contained a very considerable proportion of oxide of iron, which more or less clogged it and very materially diminished its value; therefore emery containing oxide of iron had only 20 per cent. to 40 per cent. of the value of pure alumina. The efficiency of the abrasive could not be measured by the amount of oxide of aluminium. The state of aggregation mattered very much. They might have one emery containing a great deal more alumina than another, and yet the purer alumina might not be the better grinding material.

There was a pure form of oxide of aluminium known as corundum, found in certain parts of India and America, which was a more efficient abrasive than ordinary emery. There were better forms of alumina available. There was a reaction which had come into use of late years, which consisted of mixing aluminium with oxide of iron, oxide of nickel, or oxide of anything except certain metals which were not attacked, and if they mixed aluminium with that and ignited it, it burned with such an enormous evolution of heat that the result was an oxide of aluminium, gained not at the expense of the oxygen of the air but at the expense of the oxide of iron or other metal with which it was mixed, the other metal being reduced by this action, and they had in their crucible an underlayer of metal followed by a layer of alumina. This oxide of aluminium was nothing more or less than artificial corundum, and if this artificial corundum was ground up it formed an extremely valuable abrasive. By the kindness of

the Thermite Company he was able to show them this action.

(The demonstration was then given by a representative of the Thermite Company.)

Among the artificial abrasives they had another one which held a very high place, namely, the substance known as carborundum. Carborundum was not found in nature, and was the product of a new creation of the electrical furnace. Whether it ever existed in nature before man produced it was very doubtful. It was produced at the Aitchison Electrical Furnace, Niagara. It was produced by the chemical action of carbon on sand, the combination being of silicon and carbon,

$$Si\,O_2 + 2\,C = Si\,C + CO_2,$$

which received the name of carborundum.

(Some slides showing the carborundum furnace were then exhibited.)

Proceeding, the lecturer said that carborundum possessed the property of extreme mineralogical hardness, and always breaking at extremely sharp angles, and it formed in many ways an efficient and valuable abrasive. As far as his own experience went, it was absolutely the quickest cutting abrasive he knew.

(Slides showing the carborundum powder were then shown.)

The characteristic of carborundum taking the shape of minute flat plates was rather a detriment to it, because it rendered difficult the process of sorting out the coarser grains.

After some practical demonstrations, the lecturer said he would now leave the subject of abrasives, although there was a good deal more to be said about them, and would turn to the question of polishing. The mechanism of polishing was not quite the same as abrading. There was no longer the production of minute local fractures. It would be quite possible to use an abrasive as a polishing material if they could only make the local fractures small enough. The

action of any polishing powder was partly abrasive and partly molecular action, and that was why different polishing powders differed so much in the manner of working and the results they produced.

The essential point in a polishing powder was first of all that it should be fine. The reason why extreme fineness was needed was to prevent the formation of large fractures, and in the second place to allow the powder to come into sufficiently widespread contact with the glass, in order to bring about the molecular action to which he had referred. The surface particles of the glass were torn from their allegiance to the surrounding molecules, and temporarily rendered mobile and allowed to fly about all over the surface; the result was that they spread themselves about and were able, to a certain extent, to fill up the minute irregularities of the surface and produce a polished surface such as they got on the surface of a liquid. That molecular action, he thought, would be accounted for in one particular way, and that was when the molecules of one particular substance were intruded upon by the molecules of another different substance it irritated them and rendered them restless, speaking in a very figurative sense; if they took molten lead it froze when it reached a temperature of 323° C., but if they added to it a certain proportion of tin it would only freeze at 180° C., and by adding other things they could get a substance which would melt in boiling water. This showed the effect of foreign molecules in rendering a substance more mobile.

When they were polishing a piece of glass or metal they were forcibly intruding into the surface of the work some of the polishing material. There were many phenomena connected with polishing which could be accounted for in this way. One important one was that the chemical nature of the polishing powder affected the chemical nature of the glass sur-

face which had been polished with it. They could get a kind of polish with anything. If they used an ordinary cloth and water they could produce a certain amount of polish. If they took a piece of ground glass and rubbed it with a cloth and then examined it, they would see that the higher parts of the glass had become partially polished; that was only mechanical polishing, and was not the way in which rouge and other powders acted.

A curious fact, although he did not state it absolutely as a universal fact, was that all the best polishing powders known were oxides, not salts. Now that was a very striking and very significant fact. Rouge powder, tripoli powder, oxide of iron, they were all oxides. On the other hand he had tried carbonate of magnesia, sulphate of barium, and other things which were extremely fine powder, with mineralogical hardness which ought to be quite high enough to make them polishing materials, yet they would not effectively polish. The chemical nature of the polishing material must suit the material to be polished, so that this surface inter-penetration could take place.

He would now treat each polishing material very briefly. There was rouge, which, of course, was oxide of iron, and was prepared in various ways by decomposition of salts of iron. One way was to take ferrous sulphate and heat it in an oven, when it decomposed and left behind the rouge as they knew it. The character and quality of the rouge depended altogether on the temperature at which it was heated. He was very much afraid those things were left to the workman, and the result was a very inconsistent one and often defective. He personally had examined many samples of rouge without finding a good one. The higher the temperature at which it was burnt and the longer it was burnt the harder it became, and, in fact, if they burnt it too hard it became only suitable for polishing steel. For very fine work it was

desirable to produce the rouge in a different way, namely, by precipitating in a solution of ferrous salt by means of ammonia.

Then there was putty powder, which was another well known polishing material. The better varieties were obtained by oxidising tin in nitric acid and subsequent heating, which had to be done very carefully, otherwise it was brought back to the condition of metallic tin.

Tripoli was a natural mineral which consisted essentially of silica. It was, in fact, the skeletons of infusoria, and consisted of silica in a very fine state of division. It was as a polishing medium not very satisfactory, but he believed it could be produced artificially in a satisfactory manner.

There were two other polishing materials which were very little used for glass, but could be used because they were extremely valuable and interesting in their action. The first was oxide of aluminium. In its finest and best form it was probably the most rapid and satisfactory of polishing materials for metals He thought, therefore, it was very likely it would be useful for special purposes for glass. There was another substance which was also a valuable polishing material which was not used so much for glass, but would be valuable for any substance like lime spar and rock salt, namely, chromic oxide. It was used already commercially under the name of emerald powder for the polishing of platinum by jewelers. It was obtained by calcining ammonium bichromate, which, when heated, left behind it a green powder.

Then an extremely fine abrasive was oxide of magnesia, which was prepared by precipitating magnesium carbonate and calcining it. Some of those things which had been found so very valuable in fine polishing of metals would be equally valuable in the polishing of glass.

The ordinary process of washing finely divided

powders in ordinary tap water was essentially unsatisfactory and useless, it did not produce the result desired by any means. What they had to do in order to get a satisfactory suspension and classification of their material by means of water was first of all to wash the stuff for a long time in distilled water, and get rid of all the soluble salts. If they had present in the water in which they were trying to wash a fine powder, any salt of lime, then the tendency was, under the action of this salt, for the particles of suspended matter to congregate together and fall to the bottom quickly. Therefore, they must suspend their powders in distilled water and not in tap water, and when suspended in distilled water they would further help matters by adding to the distilled water two cubic centimeters of ammonia to a litre of water. First of all the substance had to be washed with distilled water until there were no soluble salts left, then suspended in distilled water and two parts in a thousand of ammonia added; then that which was deposited in the first quarter of an hour was thrown away; that which was deposited in the first hour was suitable for polishing. In the next four hours what was deposited was suitable for finer polishes, and at the end of 24 hours they began to get a really good polishing material.

What he had put before them were simply qualitative data, but he thought it would be sufficient to arouse their interest in the question.

THE EFFECT OF ILLUMINATION ON ACUTENESS OF VISION.

By J. S. Dow.

Delivered May 25th, 1910.

Mr. J. S. Dow said that, when he was invited to say something on this subject he hardly anticipated that the remarks he had to make would be dignified by the title of a paper. He only intended to make a few suggestions, which he hoped would give rise to a stimulating discussion on this interesting topic of the effect of illumination on acuteness of vision. On many of the points involved there seemed to be a great difference of opinion.

He would like to refer mainly to various factors external to the eye which affected acuteness of vision, to speak chiefly from the physical standpoint and to emphasise the general bearing of such matters on illumination. The Society with which he was connected was very much interested in all questions connected with the distribution of light in order to reveal detail, for after all one of the chief functions of light was to enable people to read and to distinguish their surroundings clearly and definitely. One therefore naturally found that the question of the wise use of light was intimately connected with the physiology of vision. This was a very big subject, and he would prefer to speak chiefly on acuteness of vision in relation to the perception of form, rather than the power of distinguishing colour. The question of colour-perception was an intricate and difficult one, and even when one concentrated one's attention on the perception of form, the influence of the eye's

attitude towards colour was constantly cropping up.

One could divide the subject into two divisions; first of all they might consider the effect on the perception of detail of different intensities of light, and next, the question as to what kind of light gave the best results. First of all it might be asked how, exactly, was the definiteness of the form affected by the intensity of the illumination. It had occurred to him as somewhat singular that oculists paid so much attention to testing physiological defects of vision, but did not always specify the conditions of illumination under which the tests were made, because one's impressions of the appearance of detail were very much affected by the effect of the light and shade.

In perceiving form there certainly seemed to be two distinct things to consider; there was the sharpness of the outline—the definition—and there was the question of the percentage change of shade which could be appreciated. The latter, of course, was a matter which was intimately associated with the process of photometry. He had looked up the works of a number of physiologists on the subject of sharpness; practically speaking there seemed to be pretty general agreement that if one could secure an illumination of about 1—3 foot-candles for reading purposes this would answer usual requirements, and for close vision any increase of illumination beyond that point made a relatively small change in the behaviour of the eye; on the other hand, if they reduced the illumination below this value, they soon came to a point where visual acuity diminished enormously. Now, what was it, exactly, that took place in the eye to occasion this sudden fall? He had found that there was a very sudden diminution of visual acuity, indeed, somewhere about 1/5 to ½ "lux"—about 1/50th or 1/20th of a foot-candle. That point was interesting because, in

various other experiments, he had found that colour peculiarities seemed to become very pronounced at just about the same order of illumination. What was the exact nature of the change on the retina occasioning this change was a question which he felt hardly competent to discuss, but there were several theories of vision which seemed to suggest that there was a profound physiological change near this point.

Then regarding the other question of the perception of light and shade. The smallest percentage change of illumination which the eye could detect had been variously stated near one per cent. But one had every reason to expect that better results might be secured in future, because, after all, all such figures were obtained experimentally by comparing two illuminated surfaces. But an important point to be noted was the relation between this quantity and the intensity of illumination which followed a very similar course to the effect of illumination on the sharpness of form. Mr. Dow then showed a lantern slide illustrating the curve connecting intensity of illumination and the percentage change of illumination detectable by the eye. This was obtained as a result of experiments, extending over some months, with a Lummer Brodhun contrast photometer, on an ordinary photometric bench. It was found that, once an illumination over one candle-foot was arrived at, the subsequent increase had, relatively, little effect. It was also discovered that, provided that a given subjective brightness was secured, the curve followed a similar course for red, green, and white light. One could not, therefore, say that light of any colour was markedly better than any other as regards perception of light and shade. Koenig and Brodhun had arrived at a somewhat different conclusion, but Mr. Dow thought that this was to be ascribed to the fact that their results had been calculated on a theoretical law, so that the Purkinje effect was in evidence. But, properly understood and

analysed, he regarded these classic results as being in harmony with the curve presented.

This question of the best quality of light for revealing detail seemed a very intricate one. As explained, there seemed not much difference in the perception of light and shade, but as concerned sharpness, that was another matter. Unfortunately that question was nearly always mixed up with others, for example, there were peculiarities of the eye which enabled a red object to be, on the whole, better seen, in comparison with blue, when only the central part of the retina was employed. One result of that was that green and blue objects tended to disappear first as the eye receded. The two illuminated boxes on the table had been devised by Mr. Mackinney some years ago for testing questions of that kind; if the lights were turned out and they walked backwards they would get most striking changes in the relative brightness of the red and the green. Of course, there were many examples of this effect. He had heard, for example, that it was a good thing, when laying out a garden, to have the red flowers for the distant background, and only to use blue and violet shades in the front, because blue and violet did not appear noticeable at a distance. Again, one used red flags on the golf links partly for the same reason.

Influence of Intensity.—Yet, although with a strong illumination they could probably see a red thing best at a distance, they might find an exactly contrary result when they got down to an illumination at which one could scarcely see any light at all. It was well known that if the illumination was sufficiently reduced one arrived at a condition of things in which the eye was practically insensitive to red light. Under these conditions a red small spot which was faintly visible to the eye at close quarters might fade out of sight very rapidly as the eye was withdrawn, though a corresponding green spot would still be visible "out of the

tail of the eye." Some Continental experts had defined visual acuity in the terms of the distance at which a small faint source could first be made to vanish, but this involved something quite distinct from sharpness of form. Again, the result of a visual acuity test depended to some extent on the nature of the detail observed. Thus, in the case of the chess-board diagram one apparently got an additional effect, owing to the peculiar behaviour of the retina.

Next, as regards the question of sharpness of outline—definition. His impression was that it depended mainly upon the distance of the detail from the eye whether one quality of light or another gave the best result. At very close quarters it might be found that the blue end of the spectrum gave a sharp image, because, under these conditions, it was apparently easier for the eye to focus light of this kind. But when it came to distant vision the exact reverse might be found to be the case. Many people were unable to focus far away objects by blue light. However, there seems to be some people who could focus light of any colour separately, but were unable to focus two differently coloured surfaces simultaneously. In any case, he would like to point out that, for this reason, the sharpness of illuminated detail cannot be made a satisfactory basis for colour photometry. Obviously it was no use having any amount of light if the eye was unable to focus it.

Brightness and Revelation of Detail.—This question was one of considerable interest in connection with illumination. We had recently prided ourselves on the improvement in the efficiency of light production, and had been devoting attention mainly to the power of an illuminant to "create brightness." But for reading purposes we were mainly concerned with its power to "*reveal detail*," and if certain kinds of light were found to blur the image it would be better to dispense with them. In short, one might conceivably be able to see

better by cutting down the light, and it had frequently been insisted that in order to secure very sharp vision a monochromatic light was an advantage.

He would also like to mention a few illustrations of the effects of illumination upon visual acuity in practical life. It had been suggested that an illumination of up to 3 foot-candles might be regarded as sufficient for ordinary close vision. On the other hand, where very fine detail was to be studied, for example, in watch-making, textile manufacturers, etc., one might require a higher illumination than this, and a recently published report of the Conseil d'hygiene de la Seine, in Paris, had recognised this by specifying a different minimum illumination in different classes of workshops. One also knew that if the illumination was carried sufficiently high, the effect was to cause an impression of glare and to blur the retinal image, but one was not likely to reach this limit in the case of illuminated books, etc.

The effect of glare was, however, observable in other ways. For example, experiments seemed to suggest that as a rule acuteness of vision was reduced if one could see a bright source of light out of the tail of the eye while trying to read. Under these conditions the pupil aperture and the retina try to adapt themselves to the bright light so that the subjective illumination of the book becomes apparently much less. Yet Laporte and Broca had pointed out that the contraction of the pupil aperture might possibly cause an increase in the acuteness of vision though presumably only at the expense of straining the eye.

Another question was the effect on one's reading powers of the surroundings. Some observers in the United States had contended that a high general illumination led to diminution in visual acuity, and that it was better for the book or paper one was reading to be strongly illuminated and for the surroundings to be

relatively subdued. For example, Dr. Ettles had referred to the possible effect of reflection to and fro within the eye in causing a luminous haze which would overlie the retinal image. Dr. K. Stockhausen, in Germany, who held strong views regarding the injurious effects of ultra violet light, had suggested that the greater percentage of ultra violet light in modern illuminants might cause the lens to fluoresce, and thus give rise to a luminous haze which likewise impaired the sharpness of the image on the retina. Naturally, too, reflected light from a shiny surface might be very troublesome, and lead to inconvenience in reading.

Yet another question which had received some attention in the United States was as follows. Some observers were under the impression that white letters on a dark ground were much more easily read than black letters on a white ground, and had, indeed, seriously suggested the desirability of printing with white ink.

In conclusion, Mr. Dow remarked that in all the cases with which the illuminating engineer had to deal in practice he was mainly concerned with the brightness of the surface on which the detail was printed. He therefore thought that a small instrument devised by himself and Mr. V. H. Mackinney might be of interest. This instrument enabled one to judge the brightness of any surface by comparison with a standard illuminated surface in its interior. The instrument could be carried about by an observer in the street, and it was anticipated that it would be useful for examining the nature of goods in a shop-window or the condition of illumination in schoolrooms, etc. He was able to show them the first model which Mr. Conrad Beck had kindly made up for him at very short notice.

Mr. Val. H. Mackinney said: The effect on acuteness of vision of altering the colour, intensity, and kind of illumination, has received my attention from time to time. I purpose this

evening to draw your attention to two points which have not been touched upon by Mr. Dow.

In the year 1907, both before the Institute of Electrical Engineers, and the West Riding Optical Society, of Leeds, I drew attention to the fact that the natural limit placed upon the resolving power of the eye, owing to the finite size of the wave length of the light, sometimes became a factor to be dealt with when the effect of illumination on visual acuity was being considered.

You are aware that the retinal images of two distant point sources are themselves not points, but discs surrounded by alternate dark and light rings. These images are termed diffraction images, and if the first dark ring of the one overlaps the central bright disc of the other then the two objects will not be distinguishable as two. The minimum angle of resolution is that angle subtended at the back nodal point of the eye by the centres of the two bright discs, when the first dark ring of the one image overlaps the central bright disc of the other. Its measure is given by the equation $\text{Tan } \theta = m\lambda/d$ where θ is the angle above referred to, λ the wave length of the light under consideration, d the diameter of the aperture, and m that value which represents the number of wave lengths that the optical path must be increased by before the light is totally destroyed by interference. The following table, which gives values under varying conditions for this minimum angle of resolution, has been calculated by means of this equation.

Line.	Wave Length.	Minimum Angles of Resolution. 2mm.	3mm.	4mm.
B	6870	1′ 26″	0′ 57″	0′ 43″
C	6563	1′ 22″	0′ 55″	0′ 41″
D	5890	1′ 14″	0′ 49″	0′ 37″
E	5270	1′ 6″	0′ 44″	0′ 33″
F	4861	1′ 1″	0′ 40″	0′ 30″
G	4308	0′ 54″	0′ 36″	0′ 27″

The physiological limit to our resolving power is, under ordinary conditions, represented by that angle subtended at the back nodal point of the eye by two adjacent cones situated within the macula lutea region, and is of the order of 30 seconds. As nearly all of the values in the above table exceed this physiological limit, it is reasonable to assume that the diffraction images will limit the visual acuity, the effect being noticeable under certain conditions of illumination. I have brought with me this evening the apparatus with which I am able, I think, to demonstrate that the finite size of the wave length of light more often than not first limits our detail revealing powers.

As a practical instance of the value of these remarks, I would suggest that they completely explain the alleged superiority of the mercury vapour lamp for drawing office work, a fact commented upon by Professor J. Swinburne, in 1907, in his paper on "New Incandescent Lamps," read before the Institution of Electrical Engineers. The mercury vapour lamp gives a yellow-green and blue light, and, therefore, a low value for the natural limit. Moreover, the low intrinsic brilliancy of this light source assures a maximum of pupil aperture for the given foot-candles of illumination—this assists matters. In support of the contention that the mercury vapour lamp is superior in some respects to other forms of artificial lighting, I would mention that some two or three years ago I had the pleasure of assisting Mr. Dow in a number of experiments carried out at the Birkbeck Institute. Some of the results obtained have already been published, and they show that for near work green or blue light is best, while for distant work red is best; this for detail revealing purposes.

I have here another piece of apparatus which demonstrates the second point to which I wish to refer. I want to show that in an illumination problem it is not sufficient just to give so many

foot-candles of illumination, or to decide on a hard and fast rule of uniform illumination. In the two boxes shown on the table I have aimed at obtaining in one, uniform shadowless illumination, and in the other, point illumination with sharp shadows—indicating depth. Uniform illumination is obtained in this, the white box, by means of four electric glow-lamps; and in this my second box where, notice, only the floor is white, light is given out by only one source. To obtain a high degree of contrast between the shadows cast and the surrounding surface, I have used a Holophane reflector—the type which they term "focusing." Now, I think you will agree that where minute pieces of mechanism are continually being handled (for instance, by watchmakers, or those engaged in picking out types in a compositor's room) the latter kind of illumination is much to be preferred.

These remarks incidentally bring to mind how necessary it is to carefully study the subject of illuminating engineering. Further, that the particular section of this subject, which Mr. Dow has so ably opened up for discussion this evening, can well receive the attention of all, and I feel that the Illuminating Engineering Society will greatly appreciate the co-operation of this Society.

Dr. Edridge-Green said he had listened with pleasure to Mr. Dow's very interesting paper. Mr. Dow had raised a number of points of importance. He (Dr. Edridge-Green) was at present working on visual acuity in the peripheral parts of the retina. He did not know whether Mr. Dow had done any work in that direction, but the visual acuity very rapidly diminished from the centre of the eye outwards. He had also been estimating the position on the retina at which light was best seen, and although he had not made a sufficient number of experiments he found that with an ordinary white object the connected points formed an ellipse

of about four and a half by five degrees round the point of fixture.

Dr. Ettles said they had listened to a most interesting discussion on colour in its bearing on visual acuity, but one of his stumbling blocks, and one which he hoped would be cleared up in the discussion, was the equalisation of the luminosity of different coloured lights. For instance, if one said that a certain visual acuity was attained in the yellow, and a less acuity in the blue, it was quite conceivable that the brilliancy of the blue could be raised to that of the yellow and that the visual acuity might then become equal. But to him the adjustment seemed impossible, because they were incomparable.

Mr. Dow had referred to the power of some eyes of focussing either the red or the blue in dichromatic light. That showed the observer was hypermetropic. Both focal planes lay behind the retina and by a sufficient accommodative effort the blue was focused, and by a slightly stronger one the red was focused. But in the emmetropic eye the blue focus was in the vitreous and the red on the fovea, thus showing that our eyes were passively adapted to the most luminous colours. It showed also that *we were myopic for violet*, and it seemed to him (Dr. Ettles) that that point had been overlooked in studying visual acuity with homogeneous lights. The violet had to be raised enormously in brilliancy to get the same plane of vision.

Now, in even weak myopia there was great falling off in visual acuity for distance, and this could be diminished, *i.e.*, the vision could be improved to a certain extent by increasing the illumination. In his own case, while having a vision of normal for the red end of the spectrum he had to use — 0.5 D sp. to focus type illuminated with the violet end. But if that lens was worn a much less increase of violet illumination sufficed to bring vision to normal.

The influence of the pupil was a disturbing element in all this kind of work. In weak illu-

minations it dilated and exposed the basilar areas of the cornea and lens with their strong spherical aberrations; hence, while gaining in luminosity we lost in definition. By using a fixed aperture we overcame the aberration, but we lost in light perception. Again, in strong illumination a small perforation in a disc held before the eye greatly improved vision, but that was by cutting down the light intrinsically and by lessening diffusion, so that our increase in illumination was cut off from the eye, and might mean nothing at all.

As to letters on a white and black ground respectively, the speaker was quite convinced from experiment that black letters on white were far superior to white on black. With the latter, irradiation filled up the distinctive visual angles so that 6/9 of white on black could only be obtained with an eye that had 6/5 of black on white. Mr. Dow had very kindly referred to his (Dr. Ettles) explanation of glare as due to intraocular reflections. He would not however claim for it the whole cause, but he was quite certain it explained some part—thus in old age as the lens index rose the glare became much more marked. The crystalline lens undoubtedly fluoresced with ultra violet light, but that very fluorescence meant absorption, and unless that light acted on the retina it could not be seen by the person experimented on although visible to the observer.

Mr. W. A. Dixey thought he might perhaps call attention to one little point which had not been made quite clear, namely, the relation between definition and acuteness. One of the chief factors of acuteness was the degree of definition. That was always affected to a small extent by the size of the pupil; but when they got a slight degree of ametropia, as in his own case, then the difference in the acuteness which was caused by the contraction of the pupil, was quite marked. When he looked at a distant object with his myopic eye his confusion circles were

quite large enough to be appreciated, and the effect of the contraction caused by an excess of light was also quite considerable and quite easily appreciated. That point was brought to his attention some time ago, when he was trying to ascertain experimentally the best degree of illumination for test types. In experimenting he noticed one remarkable thing, that when he had a very high illumination suddenly his acuteness went up quite markedly and he attributed that to the fact that with such brilliant illumination the pupil got smaller, and so reduced the size of the confusion circles. There was one other little point in considering the effect of light on acuteness, which he did not think had been touched upon. It was a very interesting point, viz.:—The best amount of illumination for different instruments—for instance, the ordinary opera glass or field glass. It was a very important consideration how much light went into the eye. There were some purposes for which field glasses were used, where the whole point was the calculation as to how much light they could possibly get into the eye. It was an interesting point that glasses could be made too light, because if they exceeded a definite amount of light they got a contraction of the pupil and, therefore, the object of the glasses was defeated by the involuntary action of the eye. He would like to say that he thought that the solution of these problems would be easier if they were referred not to a variable thing like visual acuteness, but to a real definite value such as luminosity, if they could express luminosity in terms of the amount of light. There should be some way of expressing the amount of light independently of the effect upon the eye.

Mr. Chalmers said this might be carried beyond the point where the cornea would continue to give a good definition.

Mr. Dixey said that was a very good point, and he understood that in naval warfare in night service, what one generally wanted to see

were objects of such huge bulk that the exact definition of their form would not be a matter of great importance. When it was a question on a dark night of following the coast line which only a light-collecting instrument would enable them to see, the question was not so much one of defining a minute object, as of getting into the eye as much light as possible.

Stereoscopic Differences.

The PRESIDENT (Mr. S. D. Chalmers, M.A.) said he would like to make one or two suggestions with regard to visual acuity; under special conditions very small angular values could be distinguished. In stereoscopic vision the small differences in the angle subtended at the two eyes could be appreciated when the difference was as small as 6 seconds of arc, whereas the normal limit of visual acuity was about 35 to 40 seconds. The retina seemed to be capable of recording this small difference of 6 seconds when it occurred as a difference between the two eyes, but not in the single eye. The reason appeared to be that owing to diffraction the angular size of the image would be about 30 to 35 seconds (assuming a 4 mm. pupil). This would substantiate Mr. Mackinney's claim for the lower visual acuity limit, but would show that the single cone was capable of discriminating more finely than the diffraction limit. In this connection it was important to notice that we do consistently ignore impressions of brightness, and that the diffraction rings surrounding the image of a point must in practice be consistently ignored. Probably this had some influence on the decrease of acuity with reduced illumination. We were accustomed to ignore parts of the image of relatively low illumination, and were unable to appreciate the difference as finely as when the illumination was more intense.

Stationary Waves.

In connection with the mechanism by which light and colour were received, he (Mr.

Chalmers) would like to call attention to the analogy of the Lippmann process of colour photography. In this process the light was reflected at a metallic surface and combined with the incident light to form stationary waves with their maxima at intervals proportional to the wave lengths. In the case of the eye he would suggest that stationary waves might be formed and the motion be such as to directly stimulate the motion of the cones. One cone would then account for sensations of different colours if we could suppose that the vibration communicated at one point of a cone differed in its effects from that communicated at another point. He believed that the pigment layer might lend itself to such a reflection.

Mr. Smith asked whether it was the case that by using an optical instrument they could increase the brightness of the object. From conversations he had had with naval officers, when it was a question of trying to see a large object on a dark night they dispensed with glasses and trusted to their own eyes.

Mr. Chalmers said the thing that mattered was that they should be able to identify the ship as soon as possible, to know whether it was their own or the enemy's.

Mr. Ryland said that in correcting for myopia the patient immediately complained of glare; if they over-corrected the patient did not complain of that same phenomenon; but invariably in correcting for hypermetropia there was too much brilliancy for the eye. If there were any physiological explanation for that he would be glad to hear it.

Mr. J. S. Dow, in reply, stated that many of the points raised in his paper had been dealt with by Mr. Mackinney. He had been much interested in Mr. Mackinney's figures for the physical and physiological limits of resolution, the more so as they seemed to suggest a result which he would not have anticipated—namely, that blue light should be better than red for

fine detail. Presumably, however, the effect would be often masked by the results of the eye not being achromatic. The experiment of Mr. Mackinney on the effect of shadow was borne out by many actual points in practical experience. It seemed to be now generally admitted that a perfectly shadowless illumination was rarely satisfactory. Possibly, for very fine work, watchmaking, etc., and for the illumination of certain kinds of carving, a source of light coming as near as possible to a point would be desirable.

One point suggested by Dr. Edridge-Green's remarks seemed to call for further elucidation. How was it that, when one looked at a bright object very obliquely one lost the sense of colour? He had heard the suggestion that this was due to the fact that the rods in the eye could perceive light but could not distinguish colour; but Dr. Edridge-Green, he understood, did not believe that the rods were light-percipient at all.

Dr. Ettles had inquired how, colour-photometry being such an uncertain process, it was possible to ensure that the brightness of the background of detail illuminated by different coloured lights was the same. His answer was that though it might be impossible to compare the candle powers of heterochromatic sources in any rigid and satisfactory manner, it was, nevertheless, possible to say with fair exactitude, when two adjacent different coloured surfaces appeared to the eye equally bright, one could arrange so that a red surface was incontestibly darker than a blue one and yet the detail illuminated by the red light would be found to be the sharper and clearer of the two.

Mr. Dixey's remarks on the effect of a pupillary contraction in increasing visual acuity were interesting. He himself had noticed that short-sighted people seemed always to call for more light and to insist on a high illumination such as would be considered excessive by an ordinary

person. Possibly this might be due to the fact that a high illumination had the effect of contracting the pupil-aperture, so that the detail seen appeared sharper. Another point on which he fully agreed with Mr. Dixey was that different kinds of work required different orders of illumination. The legislation of Holland, relating to the illumination required for different trades, recognised this fact.

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